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I. THE SENSES OF THE LOWER ANIMALS.

EXPANDING the aspiration of the Scottish poet, most of us at times crave to see, not merely ourselves, but the whole world as it appears to the senses of others. We cannot help questioning what portion of our perceptions is purely objective, depending solely on the nature of the things seen, heard, or tasted, and what portion, if any, is due to our own subjectivity, and is consequently liable to vary in different individuals? For instance, we see the flower of a field poppy. It impresses our retina with a certain sensation which we have been taught to call "redness." We can find in the solar spectrum, or in Chevreul's chromatic circles, regions which make upon us the like impression. Our neighbour, unless affected with colour-blindness, gives the same name to the effect produced by the flower upon his sight.* But who guarantees, after all, that the impressions made respectively upon him and upon us are identical? Neither of us can make use of the optic nerves of the other. Much more strongely does this doubt crop up as regards the senses of smell or taste. One of the most familiar facts in daily life is to find one man praising an odour or a flavour which is to another person simply loathsome. Where lies the difference? In the very impressions made upon the nerves of smell and taste, or in the judgment formed by some inward faculty on reviewing such impressions? But if we have already grounds for question as to whether our sensuous perceptions are identical with those of our fellow-men, the difficulty becomes vastly greater when we cross the "Rubicon" of a certain learned, but unbiological, professor, and strive to form some notion of the world as it appears to the lower animals. Have they the same senses as ourselves? At the first glance the educated public will be disposed to assume that all living beings must as a matter of course be able to see, hear, smell, taste, and feel just as we do, and equally of course must possess no inlet of knowledge to which we are strangers. Closer examination, however, will throw doubts on both members of this proposition. Many animals are blind, not incidentally from disease or accident, but normally, the eyes being sometimes covered over with opaque membranes, sometimes merely rudimentary, and sometimes even totally wanting, the very optic nerve itself being obliterated. Such arrests of development occur in almost every department of the animal kingdom, even among the mammalia. Two species of mole are not figuratively but absolutely blind, *Talpa cæca*, found in Southern Europe, and the golden mole of the Cape of Good Hope (*Chrysochloris inaurata*). A rodent found in the eastern parts of Europe (*Spalax typhlus*) has eyes so small, so deeply buried in its head, and furnished with so narrow an aperture, that it may be regarded as blind, functionally if not structurally. In the caverns of Kentucky two sightless rats and two bats equally deficient are said to occur, though whether they are totally blind is somewhat doubtful. No blind bird has hitherto been discovered, but amongst amphibians the renowned *Proteus anguinus*, found in subterranean pools in the caverns of Carinthia, is a signal instance in point. Two blind fishes have also been detected, one of which, *Amblyopus cæcus*, is peculiar to the Mammoth Cave of Kentucky.

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* It has been pointed out that persons suffering from colour-blindness have often great facility in accommodating their language to that of their colour-discerning neighbours, and thus, unless systematically tested, often escape detection. Many persons who are red colour-blind do not merely fail to distinguish red from its complementary green, but red light, as light seems to make no impression on their retina—a fact of some importance.

The other senses proper to man seem to be shared at least by all the Vertebrata. They possess organs which are the homologues of our own, and which in most cases evidently discharge the same function. One exception on the large scale must not be overlooked. If we consider that smell bears upon substances either themselves aëriform or suspended in air, whilst taste relates to bodies either liquid or capable of solution in liquids, we can scarcely see how smell as differentiated from taste can exist in animals which are always immersed in water, and which respire through gills. With the exceptions thus pointed out, that of blindness in certain species and of scentlessness* in fishes, the vertebrates may be considered to see, hear, smell, taste, and feel. Along the so-called lateral line in fishes there appear to be arranged organs of sense, whose nature is not perfectly understood, but which are possibly capable of being acted upon by different kinds of undulations. Still, it would be an act of most unscientific rashness to infer that they must see the world around them as we see it. Let us take the most highly-specialised of our senses, the one most precise and definite in its indications—our light-sense. We all know how widely it varies among individual men. Almost telescopic in the Red Indian on his prairies, the Arab on the deserts, or the sailor on the ocean,* accustomed perhaps, through many successive generations to scan the horizon for indications of game, for an enemy, or of an approaching tempest, it becomes nearly microscopic in certain men of science and certain artizans constantly in the habit of scrutinising the most minute objects. It is said that Wollaston could write upon glass with a diamond characters which none of his friends could distinguish with the naked eye, but which, when examined with a good lens, were found to be beautifully distinct and regular. Nor does human eyesight differ merely in its range. Some men can discern objects clearly under a degree of illumination much more feeble than is requisite for others. Some again can detect the most minute variation in the shades of any colour, whilst to others, as Dalton and an eminent chemist and physicist now living, scarlet, green, and gray are all one.† If, then, such diversity prevails in the sense of sight within the range of our own species, if we compare the visual powers of man with those of other animals, much wider differences must be expected. Nor is this mere matter of conjecture. Direct observation proves that numerous species are able to seek their food or their prey, meet with their mates, distinguish friends from enemies, and find the way to and from their dens or nests in what to man appears complete darkness, and where he can scarcely move without running into danger. With what skill does the owl, hovering over the harvest-field, discern the grey mouse upon a soil from which it differs little in colour even by day? Other birds, on the contrary, such as the Gallinaceæ, if disturbed in the night, seem perfectly stupefied, and are utterly unable to find a way of escape from danger.

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* We want a word here. The one we use on compulsion might mean not only the inability to perceive odours, but the non-emission of any smell.

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* This last instance has latterly been called in question.

† According to the *Times of India*, a phase of colour-blindness, or, at any rate, of incapacity to distinguish colours, is very common among the lower caste natives. "Our natives cannot distinguish between blue and green. They apply the word *lal* to a variety of objects we should describe as yellow and brown, and apply the generic epithet 'tambada,' corresponding to Homer's *Chalkos*, to all the bright red tints. Like Homer, they speak of the blue sea as black (*Kala pani)*. They apply the word *nila*, dark blue, to a grey horse, and their notion of the colour of the sky, or *Asmani rung*, is a light grey. The subject can be readily tested by anyone by telling his 'boy' or some less civilised native to choose a blue, red, or green book from a pile on the table. I have just tried a puttawallah with different coloured books. Between green and blue he cannot properly distinguish; *tambada* he applies to vermillion, and the rainbow he protests is simply red or green. This is just what Mr. Gladstone says about the colour sense in Homer's Greeks."

If, then, the colour-sense in man is of recent origin, and has made notable progress since the days of Homer, the question further arises as to the date of its development among the lower animals? If the perception of colour was feeble among primeval animals their colouration must have been dull and sombre, on the hypothesis of sexual connection. Are the least splendidly coloured groups survivals of earlier days? On this subject the "stone-book"

As regards the range of vision, from what a height and to what a distance do eagles, hawks, and other diurnal birds of prey espy a suitable victim! On the other hand, if the sight of small animals is to be of much use to them, it must enable them to distinguish objects which to our unaided vision are simply imperceptible. Concerning discrimination between different colours, the evidence that it is possessed by the lower animals though indirect is conclusive. Were it otherwise the doctrine of "sexual selection," instead of being as it is, a hypothesis, not indeed demonstrated, but still conceivable, would be a simple absurdity. It is still, however, doubtful why sexual selection, if it has been a *vera causa* in the production of colour, should have led to brilliant hues in one group of birds or insects, whilst it has permitted the existence of sombre shades in another class closely allied and similar in habits. Have we here to do with a difference in the perceptive organ similar to colour-blindness, or with a difference in some inward faculty? The well-known case recorded by John Hunter where a female zebra rejected the advances of a male ass till he was painted so as to resemble a zebra is in itself quite conclusive. According to Sir S. Baker, the African elephant and rhinoceros show an especial enmity to white and grey horses, and attack them with remarkable fury. Here also belongs the antipathy of the bull and the turkey-cock to red-coloured objects. The recognition of

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throws no light, since fossils, as a rule, betray not the slightest indication of the hues they may have worn when living. We know that a preponderant proportion of the fossil insects found at Schambelen belong to the gorgeous family of the *Buprestidæ*, but even that family includes so many plainly coloured groups that no certain conclusion can hence be drawn. A parallel question of course arises as regards the fructification of flowers by the agency of insects in the primeval world.

colour by small birds generally is indisputable. Everyone must have observed with varied feelings the discrimination with which they select the "sunny side" of a pear, a plum, or a peach. It is also an established fact that they will attack the red currant in preference to the white variety, though the latter is much the sweeter of the two. Many observers during the last few years have pointed out how the yellow crocus is torn to pieces by sparrows and other birds, while the white and other varieties are unmolested. We are reminded of a curious fact which we do not know whether to ascribe to imperfect perception or to animal stupidity. A bullfinch which we kept in our chambers long ago was seized every evening with the desire to go to roost upon a line painted upon the wall about twenty inches from the ground. Time after time the poor fellow flew up only to be disappointed before he would consent to perch upon something tangible, and the next day he resumed his vain attempts.

The phenomena of mimetism or of protective resemblances, of which so many signal instances have been pointed out by Messrs. Wallace, Bates, and Belt, necessarily involve a tolerably nice discrimination of colour. Brilliantly-coloured serpents are often especially dangerous, and are hence mimicked protectively by innocent species. But unless such colouration were distinguished by mammals and birds, they would neither avoid the former nor be deterred from attacking the latter.

Caterpillars displaying gay and striking hues are generally offensive in smell and taste, or even poisonous, and are hence avoided by birds. But if the latter were incapable of recognising colours, such a conspicuous dress would not deter them from seizing the unsavoury or unwholesome morsel.

But the sight of some at least of the lower animals may very possibly differ from our own in a manner not indicated above. The human retina is sensitive only to a portion of the sun's rays. Thus, if we allow a pencil of sunlight to fall into a darkened room through a slit in the shutter and to pass through a prism, we have the well-known spectrum, all the seven primary colours of which are visible to the eye. But above and below this spectrum, where our sight finds nothing but darkness, there are solar rays whose presence may be demonstrated by appropriate chemical and physical means. Now, if the retina of any creature is sensitive to the whole or a portion of these, to us, dark rays it will be able to see where we should find total obscurity.

Again, we see all objects by means of the so-called luminous rays which they reflect. Whether they at any time reflect any of the "dark rays" is to us immaterial, because of such rays our vision is unable to take cognisance. But an animal whose retina were endowed with a wider range of sensibility would perceive the appearance of bodies modified accordingly as they reflected or absorbed such "dark rays," and would hence detect differences between substances which to our sight appear absolutely alike.

There is another point of some moment which here forces itself upon our attention. It is well known that animal organs deprived for successive generations of the opportunity of exercising their function become abortive. Of this truth several of the blind animal species enumerated above have long served as striking instances. Why should the Proteus possess an eye—a light-organ—living as he does where no ray of light ever penetrates? But as it has been pointed out by Sir Wyville Thomson in his account of the results of the *Challenger* expedition, whilst in certain animal groups permanent and total darkness leads to the total uselessness of the eyes; in others, under precisely similar circumstances, the very contrary result ensues, and the visual organs, as in the *Monida,* an animal living at the depth of 700 fathoms below the surface of the ocean, where no gleam of sunlight can reach, are "unusually developed, and apparently of great delicacy." We shall have difficulty in conceiving in a series of successive generations the continued development of an organ which is of no use in the economy of the animal. The greater weight, therefore, attaches to Sir W. Thomson's suggestion that the eyes of deep-sea creatures under such conditions may "become susceptible of the fainter light of phosphorescence." That many of the inhabitants of the ocean, both when living and when passing into decomposition, are phosphorescent to an extent visible even to eyes adapted to the stronger stimulus of direct sunlight is indisputable. The light which at certain times and places follows in the wake of a ship and marks every stroke of a boat's oars has often been described, and is due both to living animals, Salpæ, Medusæ, Pyrosomæ, Nereids, &c., and to decomposing animal matter. So powerful is this light at times that by its aid fishes have been discerned at the depth of several feet below the surface. There can hence be no doubt that an eye sensitive even to the faintest phosphorescence would be of great service to marine animals, enabling them either to discover their prey or to shun an approaching enemy.

But we have not yet done with phosphorescence. The question has often been asked why certain nocturnal animals seem greatly afraid of fires, torches, &c., whilst others, on the very contrary, are attracted by a light, and seek eagerly to plunge into it to their own destruction? One explanation proposed is that the candle or torch seems to the soaring moth an opening, or way of "escape," to which it accordingly rushes. "Escape?" we ask; wherefore, or from what? Why should the nocturnal insect which carefully shuns the daylight feel less at home in the fields or gardens by night than does the bee or the butterfly by day? An anonymous writer in "Hardwick's Science Gossip"* suggests what seems to be the true explanation. Many flowers are, even to our eyes, phosphorescent; to the vision of moths, &c., many more, if not all, possess this property, which serves to entice night-flying insects. Such creatures, attracted by what to them is the usual announcement of honey, make towards the flame, and then on nearer approach become dazzled and bewildered by its superior and unaccustomed intensity and perish in their confusion. Has this any connection with the fascination which a white cloth spread upon the ground or hung upon a tree seems to have for nocturnal insects?

It has been urged in objection that insects of carnivorous habits—amphibians, fishes, and even certain birds—are attracted by a light. Owls and nightjars have been known to flutter against the window of a lighted room in the "small hours." When sickness has been the cause of the unwonted lamp or candle, such visits have been regarded with superstitious dread. Fishes are allured to a light allowed to float along the stream or placed on the bank, a circumstance which has been taken advantage of for spearing salmon, both by poachers in the Scottish rivers and by the Red Indians in the streams of Oregon and British Columbia. A light held near to the side of a fresh-water aquarium by night brought newts, water-scorpions, and water-boatmen to the glass.

But all these facts, so far from refuting the hypothesis that moths are guided to flowers by a phosphorescent light seem to us merely to indicate the necessity for its further extension. What if phosphorescence is a very general attribute of organisms, living or dead, and if those animals which are specially attracted by a light possess eyes sufficiently sensitive for its recognition?

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* Vol. v. p. 138.

However this may be, enough has been advanced to show that the sense of sight even in animals whose eyes are perfectly homologous with our own may supply them with information which to us is inaccessible.

Very similar is the case with the sense of hearing. The human auditory nerves are sensitive to a certain range of sounds, and no further. Vibrations either more or less rapid do not to us break silence. But we have no right to infer that they may not be distinctly heard by other animals. That they actually are perceived has in many cases been shown by direct experiment with the instrument known as "Galton's whistle." Upon this a line is marked indicating the usual upper limit of the sense of sound in the human species, and corresponding to from 41,000 to 42,000 vibrations per second. But when this limit is passed, and when the whistle no longer produces a sound appreciable to man, several animals still indicate by their movements that they hear. Cats, birds, and some insects seem decidedly more affected by high than low tones. In *Sphinx Ligustri* and *Metopsilis Elpenor* the reverse is said to hold good. In short, we may conclude that many living creatures recognise sounds which utterly escape our ears, and thus receive through the medium of the sense of hearing impressions perhaps more varied and numerous than, or at least different from, our own. A wonderful instance of combined delicacy and discrimination in the recognition of sounds is shown by deer in America. It has been repeatedly observed that if one of these animals is browsing in a forest in windy weather, when the trees are groaning and creaking to the blast, and branches are occasionally snapping, yet, if amidst all this uproar a rotten twig happens to crack beneath the foot of an approaching hunter, the animal at once stops feeding, gazes carefully around in every direction, and remains unusually suspicious often for hours. The ears which discriminate so nicely must be uncommonly sensitive.

We come now to scent, which is to civilised man probably the vaguest of all the senses in its indications. It may yield us a certain amount of pleasure and a very palpable amount of annoyance. It may warn us against eating, touching, or approaching certain noisome or putrescent substances; but in this respect its warnings are not trustworthy. It is far from being proved, and indeed is highly doubtful, that the stage of animal or vegetable decomposition most offensive to our nostrils is the one which presents the greatest amount of danger. The most deadly atmosphere of the

Terai, of the Gold-coast, or of the Tierras Calientes smells to us exactly like pure, ordinary, wholesome air. On the other hand, a wholesome and even delicious fruit, like the durrian, may have a most disgustingly repulsive odour. Nor can we by this sense discover what track a missing friend has taken, or pursue the footsteps of an enemy. In the lower Mammalia this is almost reversed. Concerning the delicacy of scent in dogs many interesting facts have been brought forward, and not a little "tall talk" has been perpetrated. But their scent differs from our own not merely in its power of detecting odours which escape us altogether; they are evidently gratified by the smell of carrion, of ordure, of animal excretions and secretions, which to us are utterly offensive. But if the scent of dogs has attracted more prominent attention from the circumstance that it is at the disposal of man in hunting, that of other Mammalia is little, if at all, inferior. Animals of the weasel tribe pursue their prey by scent, and even hunt in packs. Swine can follow their companions by the same clue, and, as it is well known, have been trained to discover truffles under ground. Elephants, deer, bears, and in fact many other wild animals, can only be approached with safety from the lee-side, as they otherwise soon become aware that an enemy is at hand. Rats have a delicate scent, and in setting traps or poison for them it is necessary to avoid touching the bait with the hand; otherwise their suspicions are at once aroused. Almost all animals seem to know—at least as far as the productions of their own locality are concerned—by the odour whether any substance is suitable for food, and this fact has accordingly been enlarged upon as a marvellous case of "instinct." Very many species are also decidedly attracted by odours which have no connection with their food, and which are to them, therefore, a mere luxury. The fondness for valerian, lemon-thyme, chamomile, lavender, and many plants rich in essential oils, is common to the whole feline family. On the other hand, they have their dislikes. We have often observed the domestic cat smell at a fig-tree, and turn away with the air of a disgusted connoisseur. Carbolic acid and the coal-tar products generally seem to be an abomination to everything that comes under the head of "vermin." Oil of rhodium, on the contrary, has a wonderful fascination for rats and mice, and is said to be a prominent ingredient in the mixture which enables the modern housebreaker to lull even the most vigilant watch-dog into a temporary neglect of duty.

The question of the scent of the vulture, so hotly discussed by Waterton, Audubon, and others, can scarcely be regarded as definitely settled.

Serpents have a more powerful sense of smell than is generally imagined. They do not, indeed, pursue their prey by scent,—or, rather, they do not pursue them at all,—but lie in wait, and seize any suitable victim that presents itself. But we have been repeatedly struck with the commotion excited in a reptile vivarium on the introduction of a mouse, shrew, or other small warm-blooded animal; whilst the arrival of a frog, toad, newt, or lizard passed quite unnoticed. Our observations lead us to believe that among vipers the male, who is of wandering habits, seeks the more sedentary female by scent. Some country people hold that serpents are remarkably fond of milk, and will follow by scent a farm-labourer who is carrying a full pail from the fields, or even a woman who is suckling her child. But we never met with any authenticated facts in support of this tradition.

Fishes are, as is well known, attracted by various substances thrown into the water; but it can scarcely be said that, in their case, the two senses of smell and of taste are thoroughly differentiated.

Of the sense of taste in animals we know but little, save that many species are exceedingly nice in the selection of their food, and reject with considerable obstinacy an article which does not meet their requirements. Thus an owl can rarely be induced to eat meat in the slightest degree tainted. This should argue no small delicacy of taste. On the other hand, those creatures which swallow their food whole—such as serpents, lizards, frogs, toads, and many birds—can scarcely be supposed to recognise its taste. We have heard of a python, in a fit of hunger, swallowing a blanket.

Touch, the least localised of our senses, is too often confounded, in popular apprehension, with feeling. But surely the mere recognition of resistance, in certain directions and to various degrees, has no necessary connection with the idea of pleasure or pain possibly excited in the system. This is a fortunate circumstance, as it saves us from discussing the vexed question as to how far and to what extent feeling extends down the animal, or even the vegetable, kingdom. But that touch exists in all animals will scarcely be disputed. Its seat, however, varies greatly in different groups. The hands or fore feet in the Primates and Carnivora, aided in many of the latter by the whiskers; the snout in the lower mammals, the beak apparently in birds, the tongue in many reptiles, the feet (sometimes aided by the antennæ) in articulate animals—all exercise this function. With the exception of a doubtful case in bats, to which we shall have to refer below, we doubt if any of the Vertebrates possess the sense of touch in as high a degree of perfection as does man.

Reviewing the facts already adduced, we are surely warranted in concluding that, even as regards those animals which possess organs of sense clearly homologous with our own, and exercising demonstrably the same function, the sense-perceptions are not necessarily the same as our own, the probability being that—so far at least as sight, smell, and hearing are concerned—we are surpassed by not a few of our humbler fellow-tenants of the globe. They may, or rather they actually do, see where to us there is mere darkness or a void, hear where we find utter silence, smell what to us is inodorous, and distinguish grades and modifications where our senses pronounce there to be no difference. In fact, just as our bodily nakedness, slowness, feebleness, and lack of weapons require to be supplemented by clothing, vehicles, arms, and machinery, so our dull senses demand the aid of the telescope, the microscope, the spectroscope, and other the like aids.

But we have still to consider those beings whose organs of sensation are not homologous with our own, and exercise function as yet imperfectly ascertained. Above all, we have to keep in view the possibility that certain animals may enjoy senses whose nature is to us altogether unknown. To deny, *a priori*, the existence of such senses is as if we were to assert that because we possess no poison-fangs, therefore the bite of the cobra is harmless,—or that because we do not secrete silk, the spider and the caterpillar are unable to spin. It is, in short, that ever-besetting error "Man the measure of all things."

Taking up first the latter question, we will call attention to the wonderful dexterity with which a bat will flit about in a locality full of prominent or pendent objects, without coming in collision with any. It is sight, you say? But an experiment has been tried which will, we fear, be pronounced a case of "violationism." A bat has been deprived of sight, and turned loose in a room where a number of rods, strings, and other objects were suspended from the ceiling; but it avoided these obstacles just as well as if still possessed of its eyes. Feeling or touch? We know that men perfectly blind, when moving slowly along, can judge whether they are approaching an object, such as a wall.

But here is a creature darting rapidly about, and yet able to turn and wind between a number of small obstacles. If this is touch, seated in the wings, in the lobes of the ears, or in the leaf-like appendages which in some species adorn the end of the nose, it is so highly subtilised as almost to deserve the rank of a distinct and independent sense, fulfilling, as it does, functions for which the ordinary feeling or touch of man and other animals is as impotent as it would be for recognising a colour or a sound.

The possibility of other senses than the five with which we are endowed will at once appear if we reflect that of all the so-called "physical forces" light is the only one of which we have a direct perception. We only recognise heat and electricity when they give rise to some phenomenon which appeals to our sight and our feeling. Now, it is plain that senses may exist which take direct cognizance of the electric or thermic state of bodies. Had we such senses they would evidently enable us at a glance to distinguish bodies which, without formal scientific examination, appear to us identical, and to recognise changes of condition which now escape us. An animal possessed of a magnetic sense would, for instance, be able, without any mental effort, to direct its course northwards or southwards, and in crossing an unknown region would enjoy all the advantages which we derive from the use of the compass.

The possibility of new, undiscovered senses is of course greatest in the Invertebrates, and chiefly in the Articulates. We find these creatures executing actions which from our point of view demand reason of a higher grade than we are willing to concede to beings so widely different from ourselves, and—what is much more to the purpose—in which we fail to trace in other points such effects as we might naturally expect to flow from the possession of a highly-developed intelligence. There appears in their economy a mixture of wisdom and folly more incongruous than we at least can imagine may be detected by higher beings in our own. When, further, we examine their structure, we find organs of sensation upon whose functions we are far from being able to pronounce with full certainty.

Concerning the organs of sight in insects there can fortunately be no dispute, but their eyes differ so widely in structure from those of the vertebrate animals as to offer not a few unsolved problems.

Many groups possess two distinct kinds of eyes—the larger or compound, and the smaller, simple, or so-called *ocelli*. The former—found in all mature insects, save certain blind species—occupy a position similar to that of the eyes in the Vertebrates, but, instead of consisting each of a simple lens, they are formed of an aggregation of lenses, varying greatly in number, and in some groups amounting to many thousands. From each of these lenses or facets a crystalline rod radiates downward to the nervous ganglia. The *ocelli*, or small eyes, placed on the top of the head are simple in structure, absent entirely in many species, and in others covered over with hair.

We can scarcely doubt that a structure so widely differing from that which prevails among the higher animals must be accompanied with corresponding differences in function to some of which we have already referred. We know that the sight of many insects must require a wide range. If they are to find their way back to their nests, hives, or other haunts, they must be able to recognise objects at a very considerable distance, whilst in other cases their vision to be useful must be almost microscopic in its character. In accordance with this twofold need it has been observed that in many groups the upper lenses of the facetted eyes are considerably larger than the lower. Hence the former may possibly serve for the recognition of distant objects, and the latter for the examination of such as are near at hand.

Graber, however, maintains that the compound eyes are telescopic in their function, whilst the *ocelli* are adapted solely for the perception of proximate objects, whence their strong convexity. He remarks that these simple eyes occur especially in insects whose locomotive powers are feeble, and whose entire sphere of action is restricted. To this view exception must be taken: the Neuroptera, such as the dragonflies,—perhaps the most locomotive of all insects,—the great majority of the Hymenoptera and Diptera, all of them swift, strong, and agile on the wing, possess these organs; whilst in the Coleoptera, comparatively clumsy and imperfect flyers, they are generally absent.

The *ocelli*, in the Hymenoptera at least, are, according to F. Müller, adapted to a very feeble illumination, their size increasing as the habits of the species are more nocturnal.

But even insects in which no eyes can be traced—such as the blind grubs of *Lucilia Cæsar, L. cristalis*, and other flies—have, according to Pouchet, a perception of the intensity and the direction of incident rays of light, diffused apparently over the whole surface of the body.

The eyes of spiders are not facetted like those of insects, and occupy a position more corresponding to the ocelli than to the compound eyes of insects. This position, awkward as it may seem at first glance, is a great convenience to the spider, whose enemies—such as birds and certain wasps—are sure to attack from above, and whose cell or hiding-place is generally underneath the web. From our own observations we do not think that the sight of spiders has a very long range.

Insects are sometimes led astray by their unaided sight. The "humming-bird hawk-moth" has been seen examining artificial flowers on a lady's bonnet, and even the coloured designs on walls,—a fact which confirms the attractive influence of colour upon insects, and corroborates Mr. Darwin's views on the fecundation of flowers.

The light-organs of snails offer most interesting peculiarities. In the genus *Oncidium*, found in the Philippine Islands, and recently studied by Dr. Semper, there are upon the back small specks, which are in reality eyes, essentially similar in structure to those of vertebrate animals, and quite distinct from the well-known tentacular eyes. What may be the difference of function in the two organs has not yet been ascertained.

Passing to the sense of smell, we may be met at the very outset by the inquiry whether insects have any perception of odours at all? This question is of great moment in the general economy of organic Nature. According to several modern investigators, among whom we may especially mention Darwin, the fecundation of flowers, and consequently the propagation of vegetable species, depends in a large number of cases upon the intervention of moths, butterflies, bees, and other insects, which convey the pollen from the male organs of one flower to the female organs of another. These creatures are attracted to blossoms, in some cases of brilliant colour and in others of odour, this being in fact the final cause of such beauty and fragrance. If, therefore, insects are not endowed with the sense of smell, one part, at any rate, of this theory must be given up, and the perfume of flowers must—as far as the welfare of the plant itself is concerned—be pronounced purposeless. It will therefore be useful to review the evidence which proves the existence of an acute and delicate odour-sense in insects, the more as a recent experiment has been by some writers supposed to demonstrate its absence.

We will first glance at some of "Nature's scavengers," such as the sexton-beetle and the dung-beetles. One of the most familiar facts in the economy of these creatures is the ease and certainty with which they discover their quarry. On a calm spring evening nothing is more common than to see a *Geotrupes* come flying along in a straight line, not hawking or searching about, and drop at once upon some recently deposited ordure. Where burying beetles are common they may be seen, in like manner, flying one after another to some dead mole, or shrew, lying amidst high grass, and certainly incapable of being seen from a distance. Every housewife must have noticed with what importunate assiduity the common blow-fly will hover about a cupboard or safe enclosing meat, even when the contents are totally hidden from view. It is especially interesting to note that certain plants which to our senses emit the odour of carrion prove attractive to carrion-flies. It is less generally known that some of the most beautiful butterflies are attracted by excrementitious and putrescent matters, and may even be lured within reach of the entomologist's net by such baits. This scarcely agrees with the poetical notions of butterfly-life; but alas! *Psyche* will sip the fœtid moisture from carrion as eagerly as the nectar from the purest flower. A dead weasel or rat nailed against a tree-trunk will often induce the "purple emperor" to descend from the tree-tops over which he is wont to flutter. The *Papilios* and giant *Ornithopteras* of warmer climates may also be captured by similar stratagems. According to Mr. W. M. Gabb,* the brilliant *Morphos* of Nicaragua may be caught "by baiting with a piece of over-ripe or even rotting banana," whilst at other times they were almost unapproachable. The same author adds that his native servants "always carried with them a fermented paste of maize-flour, which they mixed with water to the consistency of gruel, as a beverage. On our arriving at the side of a stream in a narrow gorge, invariably, within a few minutes after they opened a package of this paste, although there might not have been a butterfly in sight before, these most brilliant of their kind would come sailing up, always from the leeward." Our common *Vanessa Atalanta* is, in like manner, attracted by the smell of over-ripe or rotting fruit, especially plums.

One of the commonest and most successful methods of capturing nocturnal Lepidoptera, *i.e.*, sugaring, depends on an appeal to their sense of smell. A thick syrup of coarse brown sugar is mixed by some experts with rum, by others with porter, and by others again with a little vinegar,—all strong odorous fluids,—and the composition is smeared upon the trunks of trees. The moths come to sip the syrup, and are caught.

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* Nature, February 7, 1878.

But there is another method of catching Lepidoptera which is even yet more convincingly demonstrative of the wonderfully acute scent possessed by these creatures. We refer to the practice of "sembling." If a virgin female moth of certain species is shut up in a box, males of the same species will make their appearance, even from a very considerable distance. Thus Mr. Wonfor, in a paper read before the Brighton and Sussex Natural History Society, mentioned that he had by this method captured, in two days, fifty males of *Saturnia Carpini*. He declares that the attraction "cannot be by sight, for the females were in a box on the side of a slope, and the males flew across the valley and close to the ground. When trying similar experiments with other species we purposely selected a field with a wood at the end, and saw the males flying over the tops of the trees." They always, further, approach against the wind. Two additional circumstances have to be taken into consideration: as soon as the female is impregnated the attraction ceases, and, further, the moths in question are by no means common. In the same district where Mr. Wonfor made his experiments any person, not having with him a female *S. Carpini*, could scarcely count upon meeting with a single male of the species in the course of a day's ramble.

The following instance, recorded by Mr. J. H. Davis, Curator of the Portsmouth Philosophical Society,* is conclusive against the supposition that the males are attracted to the female in consequence of discerning her afar off:—"Another female of the same species (*Sphinx Convolvuli*) had been produced; three males found their way into my study down the chimney." Many more instances of male moths coming and hovering round, or settling upon perfectly opaque boxes in which females of their own species were imprisoned, might be adduced did any necessity exist. It is, however, contended by some that the attraction, though not sight, may be a sound. The virgin female, they argue, produces a sound inaudible to human ears, but distinctly heard and understood by the males of her species, and becomes silent as soon as her love-call has been answered. There is not in this supposition anything necessarily absurd; but the following fact proves it to be utterly inadmissible. Mr. J. H. Davis, in the Journal above cited, tells us:—"On going into my study, in the evening, I found a female *Sphinx Convolvuli* fluttering on the floor. On lifting it up it ran up my coat and several times round the collar before I could place it in safety. I went from thence into my garden, to shut some hot-bed lights, where I was occupied about ten minutes; from thence again to my study, where I found two fine males of *Sphinx Convolvuli* had, whilst in the garden, attached themselves to the collar of my coat, where the female had previously been." This instance, we think, is absolutely decisive. A scent might be easily left adhering to the collar of the coat, whilst no one can conceive of a sound remaining. Male moths have further been known to flock to the empty cocoon from which a female had recently escaped, though she had in the meantime been removed. Sight and hearing being evidently thus out of the question, there remains only one sense known to us capable of acting at a distance. Judging from our own powers, indeed, scent of such delicacy and subtlety is simply incomprehensible. We wonder at the accuracy with which the harrier can track a hare, or the bloodhound a fugitive criminal or slave, over meadows, marshes, ploughed lands, and trodden roads; and if common observation were not against us we should doubtless proclaim this also inconceivable or impossible. But the task of the hound is vastly easier than that of the moth. He is guided by scented surfaces to which the specific odour of the animal pursued adheres in a concentrated form. The emanations from the female moth, on the other hand, are diffused through space and diluted with an excessive quantity of air, whether scentless or saturated with other odours. Yet this infinitesimal trace is sufficient to guide the male towards her with unfailing accuracy, and from distances of at least 200 yards, or about 3600 times the length of the insect's body. This is as if a human being were able to detect the presence and the condition of an individual of his own species at the distance of more than four miles! Inconceivable, however, as such a power may seem to us, there stand the facts before us, and we have merely the option of admitting this wonderful delicacy of scent or assuming the existence of some sense totally foreign to us, and possessed of equal delicacy in its indications.

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* Zoological Journal, vol. v., p. 142.

According to the observations of Mr. Belt,* confirmed by those of others, the *Ecitons* and other ants follow each other by scent. Each exploring party marks out thus the road it has travelled. He says:—"I one day saw a column of *Eciton hamata* running along the foot of a nearly perpendicular tramway-cutting, the side of which was about 6 feet high. At one point I noticed a sort of assembly, of about a dozen individuals, that appeared in consultation. Suddenly one ant left the conclave, and ran with great speed up the perpendicular face of the cutting without stopping. It was followed by others, which, however, did not keep straight on like the first, but ran a short way, then returned, then again followed a little farther than the first time. They were evidently scenting the trail of the pioneer, and making it permanently recognisable. These ants followed the exact line taken by the first one, though it was far out of sight. Wherever it had made a slight *détour*, they did so likewise. I scraped with my knife a small portion of the clay on the trail, and the ants were completely at fault for a time which way to go. Those ascending and those descending stopped at the scraped portion, and made short circuits until they hit the scented trail again, when all their hesitation vanished, and they ran up and down it with the greatest confidence." Mr. Belt further thinks that ants "can communicate the presence of danger, of booty, or other intelligence, to a distance, by the different intensity of the odours given off." In this hypothesis of a scent-language addressed to the organs of smell, taking the place of a sound-language addressed to the organs of hearing, strange as it may seem at first glance, there is nothing impossible or even improbable. The language of man and of many other Vertebrates demands the power of producing at will sounds, joined to the faculty of recognising them ; in other words, the joint possession of vocal and auditory organs, In like manner, the language which Mr. Belt thus attributes to ants requires merely the power of producing odours, and of distinguishing them when produced. Now, one of the most striking peculiarities of insects, as compared with vertebrate animals, is the variety and intensity of the odours which they emit, even as appreciable to our olfactory nerves. There are genera, and even species, which an experienced entomologist can recognise, even blindfold, by the smell alone. How distinct these scents must be to such sensitive organs as are evidently possessed by insects will be understood from the facts stated above. It appears also that in certain cases these odours can be emitted, suppressed, or varied at will. Here, therefore, are all the facilities needed for a scent-language. Our want of some standard for remembering and recording odours renders the study of this subject peculiarly difficult.

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* Naturalist in Nicaragua, p. 23.

Curiously enough, the notion has been lately taken up that insects, after all, do not possess the sense of smell.

An experimentalist placed some caustic ammonia close to the head of a moth which was either asleep or "shamming" death, and was surprised to find that it took little or no notice of the pungent vapour. On the other hand, he observed that a loud and sudden sound seemed to startle the moth. To do the writer in question justice, he does not appear to have drawn from this single experiment the rash and sweeping conclusion that insects are incapable of smelling. Others, however, were less cautious, and not a few paragraphs have appeared in consequence, in political and literary journals, to the effect that the supposed possession by insects of a sense of smell must now be regarded as an exploded error. We have found, however, not indeed an insect, but a spider perfectly sensitive to the odour of ammonia. Being once annoyed out of all patience by a *Tegenaria*, which would persist in attaching her web to a burette-stand in the window of our laboratory, we unstoppered a 20-oz. phial of the strongest ammonia, and presented it at her, in the hope of putting her to final rout. With the usual courage of her race she charged the strange object with so much eagerness as nearly to fall into the open bottle. Catching the fume, however, she at once turned tail, and fled precipitately. Perhaps if we fully understood the structure of the olfactory organs in moths we should see a reason why they are little affected by a dose of ammonia from which we should shrink back half-stifled. Their nerves of smell do not appear to terminate in a mucous membrane liable to be irritated by ammonia. Furthermore, we have observed cases where odours repulsive to man appear indifferent, or even attractive, to insects. That gnats possess the power of smell is demonstrated beyond all reach of doubt; yet we have seen a crowd of these little beings dancing merrily over the ventilators of a shed from which the orange fumes of hyponitric acid were escaping in torrents. It would have been easy for them to have found an equally eligible place free from the pungent vapour, but they showed no disposition to withdraw. In summer mornings, also, we have frequently observed moths of various kinds drowned in bowls of solution of tin in *aqua regia*, whilst none of these nightly visitors had thought proper to commit suicide in a cistern of water close at hand. We were hence led to conclude that the odour of the tin solution was to them positively attractive. Such solutions indeed have, under certain circumstances, a faintly aromatic smell which might be likened to that of decaying fruit.

But if insects thus—as we are warranted in concluding—

possess the sense of smell in a degree of perfection quite unknown to vertebrate animals, the question follows, What is its organ? Here there is a want of accord among authorities who have made this subject their study. Still we hold that a preponderating body of evidence points to the antennæ as the seat of this sense. These organs occupy a situation exceedingly appropriate for the purpose; they are exposed to currents of air, and can be readily applied to or held over any substance which the insect may wish to examine more closely, and they are most abundantly supplied with nerve-fibre. In flies the third or terminal joint receives thousands of such filaments, each apparently terminating in a small open cell. In some of the Buprestidæ the antennæ display multitudes of pores or open cells, scattered uniformly over the entire surface, whilst in others they are concentrated in a small depression upon each joint. The development of the antennæ, which varies greatly in different groups, presents, in the main, the features which we should expect in an organ of scent. We know that in insects—as indeed in all animals—the male seeks out the female, who in most cases is more sedentary in her habits, and is in some instances even devoid of wings. Hence we may fairly conclude that the male will require to possess the sense of smell in a higher degree than the female. This is accordingly the invariable rule: if the antennæ of the two sexes are not absolutely alike, those of the male are more highly developed. This is especially the case in such moths as are usually entrapped by the process of "sembling," as above mentioned. In these, as for instance in *Saturnia Carpini*, the antennæ of the female have the form of a bristle, while those of the male have a series of minute plates, like the barbs of a feather, projecting out on both sides, and exposing a great amount of surface to the air.

We should also expect the organs of smell to be more complicated in species which feed on a very narrow range of substances, and have consequently more difficulty in finding support than in omnivorous species whose food is everywhere. We should also suppose that a less delicate sense of smell, and consequently a less highly developed organ, would be necessary in insects possessing great locomotive powers than in such as travel slowly and awkwardly. Further, if the antennæ are the organs of smell, their development should be to some extent inversely as that of the eye, and should be relatively higher in nocturnal than in diurnal species. All these suppositions may, generally speaking, be pronounced to agree with observed facts. The dragonfly, with his wonderful speed and command of wing, and with his scarcely less marvellous development of the visual organs, has comparatively little need for the sense of smell, and his antennæ are small and simple. The locust, strong on the wing, able as a leaper, and prepared to eat almost every green thing upon the face of the earth, has small occasion for minute discrimination of odours: his antennæ, accordingly, are mere bristles. The tiger-beetle—able to run, bound, and fly with extreme velocity, and preying upon every animal he is able to overcome—needs little delicacy of scent, and his antennæ therefore are of the same simple type. The ground-beetles, though in many cases wingless, and in others nocturnal, are willing to feed both on living prey or dead animal matter, and in case of need upon certain vegetable substances. We need therefore feel little surprise that their antennæ, too, should be plain in structure. The common house-fly is swift and powerful on the wing and unlimited in its diet; it therefore has less need for a nicely appreciative scent, and in consequence for highly developed antennæ.

On the other hand, the male moth who has to seek both his food and his partner, often in the night, has in a majority of instances highly developed plumose antennæ. The butterfly—who, though feeding on the honey of flowers, does not appear to be promiscuously attracted by all plants—has antennæ furnished at the end with buttons or knobs. The common dung-beetle, who crawls slowly and flies heavily, and who depends upon one class of substances alone for his own food and for the *nidus* of his offspring, has at the end of his antennæ a club that opens out in plates, like the leaves of a book, and thus exposes a very large amount of surface to the action of the atmosphere.

We may further remark that the antennæ in larvæ are in a rudimentary state, and merely become developed when the insect has reached the reproductive stage of its existence.

This also is in favour of our view that they are the organs of scent—a sense which throughout the animal kingdom seems to stand in a close and particular relation to the sexual functions. It has been observed that in our own species the olfactory nerves are comparatively inactive prior to the age of puberty.

The question has often been raised—By what means does the ichneumon-wasp, or other parasitical insect, discover the presence of the larvæ or pupæ destined to become its victims, hidden, as the latter frequently are, among closely folded leaves, or in fruits, in the stems of vegetables, or in masses of earth? The sense of smell seems by far the most likely guide; and if we watch a female ichneumon on the search for larvæ in which to deposit her eggs, and note the rapid and systematic play of her long flexible antennæ over the surface of the objects she is examining, we cannot help comparing her movements to those of a hound searching for the trail of a fox or a deer. If we admit that the antennæ are the olfactory organs all becomes intelligible.

Actual observation, as far as it has been carried, testifies in the same direction. We have often offered fruit or flowers to captive rose-beetles, and have always found that their first action was to stretch out the antennæ and expand the leaflets of their terminal clubs. Dung-beetles act precisely in the same manner if suddenly presented with a piece of excrement. Indeed all insects whose antennæ are large and conspicuous enough to be conveniently observed act as if these organs played a most prominent part in the recognition of food placed in their way. That they may be, at the same time, organs of touch is perfectly possible. The proboscis of the elephant, the snout of the swine, &c., fulfil this double function.

Provisionally, then, we think it may be admitted that the antennæ are the organs of smell. But till we are able to show some correlation between the form of these organs in each group and its peculiar habits and requirements, or its general structure, our knowledge must be confessed to be exceedingly imperfect. We are perfectly aware of Dr. Wolf's supposed discovery of an organ of scent in insects, which is merely a "specially differentiated portion of the membrane" which extends from the labrum inwards. We admit that the part examined by this naturalist is an organ of sensation. But we do not see that even the attempt has been made to trace any connection between its development and the varying degrees of olfactory power.

Though second in moment among the senses in man, the faculty of hearing must, among invertebrate animals, receive a lower position in accordance with the part which it plays in their economy. On this subject, however, we are almost daily receiving new and often startling revelations. Naturalists have long known that some insects possessed the power of producing sounds at pleasure, and have very justifiably inferred that such species cannot be devoid of the sense of hearing. The responsive chirp of the cricket, the cicada, and the grasshopper; the peculiar note uttered by the queen bee, and which produces such an effect upon her subjects; the importunate or querulous hum of many Hymenoptera when angry; and the wailing buzz of the common house-fly when captured in the web of a spider, on hearing which all other flies beat a retreat from the spot—all these instances of insect-voices and insect-hearing are well known. But till very lately the vast majority of insects and of other articulate animals were considered literally dumb. Now, however, vocal powers are being discovered in spiders, scorpions, butterflies (hair-streaks), moths, beetles (*Cychrus*, *Prionus*, &c.), as well as in the groups formerly known to be noisy. Several *Vanessæ*—members of the group to which our common "peacock butterfly" and "red admiral" belong—are known to stridulate, as also the Brazilian butterfly (*Ageronia feronia*), and the moths *Chelonia pudica* and *Euprepia matronula*. The "death's-head" (*Sphinx Atropos*) was formerly supposed to be the only Lepidopterous insect capable of emitting any sound. The organs of sound are often very curiously constructed, and are provided in some cases with a resonant cavity for the purpose of intensifying the effect. If we further take into consideration the circumstance that the sounds produced by such minute animals may easily be too acute to be recognised by human ears, we shall not be far wrong in supposing that the majority of the Articulata can emit sounds at will, and that they therefore are probably endowed with the sense of hearing.

It is to be remarked, however, that the recent discoveries of the production of sound by insects refer more to the solitary species than to such as live in organised societies. Ants have not hitherto been observed to utter any sound, save a kind of hissing when on the march. Their antennal language—whether or not we regard it, with Mr. Belt, as depending upon the production and recognition of odours, or view it merely as a system of movements and touches, somewhat resembling our "deaf and dumb alphabet"—can scarcely be referred to sounds. The stridulation of solitary insects is often, doubtless, a love-call; often again, as in the scorpion, a note of defiance. Predatory insects are doubtless, like larger animals, often guided to their prey by the sense of hearing, and the feebler species may in some cases be made aware of the approach of danger. Gilbert White* curiously enough accuses bees of deafness:—"It does no appear from experiment that bees are in any way capable o being affected by sounds; for I have often tried my own with a large speaking-trumpet held close to their hives, and with such an exertion of voice as would have hailed a ship at the distance of a mile, and still these insects pursued their various employments undisturbed, and without showing the least sensibility or resentment." The rustics who, when pursuing a stray swarm of bees, kept up a horrible dissonance with rattles, cows' horns, frying-pans clashed together, and the like, seem to have taken a different view of the hearing of bees.

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* Selborne, Letter XXXVIII.

The question as to the ears of insects is not satisfactorily settled. It would almost seem that these organs, or what stands in their stead, are placed differently in different groups. In the two-winged flies (Diptera) the so-called poisers or halteres—the small knobs which take the place of the posterior wings of other insect-orders—have been supposed to be the organs of hearing. The Orthoptera are said to have ears on their fore-legs, and other insects seem to possess similar organs in the subcostal vein of the wing. It may be asked, how can these points be ascertained? There is certainly, in observing and experimenting on such subjects, wide scope for error. We once saw it gravely maintained that the antennæ of insects served as auditory organs because when an insect is startled by a loud and sudden sound a convulsive movement is sometimes observed in these members. This, however, proves nothing: a man under similar circumstances will often give a sudden jerk with his arms; yet no one will maintain, on that account, that we hear with our hands. To recognise the organs of sensation in the vertebrate animals is generally easy, because they occupy positions answering to those which they hold in our own system. But among Invertebrates the case is different: organs which in mammals, birds, &c., are concentrated in the head or on the trunk, may there appear on the limbs. If then we find, *e.g.*, on the wing or the leg of an insect some apparatus specially supplied with nerves, and yet obviously adapted neither for locomotive, prehensile, vocal, secretive, or reproductive functions, &c., we arrive, by a process of exhaustion, at the inference that it is most probably an organ of sensation. Concerning the eyes there can be fortunately no doubt. The senses of smell and taste we may reasonably expect to be in close proximity to the mouth. So that a highly specialised organisation found on the wing or leg, as above mentioned, may with great probability be pronounced either to be an ear or the seat of some sense totally unknown. If, on experiment, the removal of such organ is found to bring with it the incapacity to recognise sounds, the evidence may be considered as complete.

Concerning taste in the Invertebrates we know very little beyond the bare fact that many of them are extremely scrupulous in the selection of their food, and will in many instances accept death by hunger as a preferable alternative to a change of diet. Here, however, the functions of smell and of taste are so interwoven that we are not yet competent to draw the boundary line. What are the organs of taste in insects is still a matter of doubt; the palpi, the so-called tongue, and the inner surface of the alimentary aperture have all been suggested.

Touch plays a very important part in the economy of the Articulates, and attains in some of them a delicacy at least as great as in man. Pope's assertion that the spider's touch "lives along the line" is the expression of a literal truth. If an *Epeira* is sitting in the centre of her geometrical web, and a gnat becomes entangled towards the circumference, she may be seen applying her feet in succession to the different radii, and then bounding off in the right direction. That she is guided by touch rather than by sight appears from the circumstance that she may be induced to rush out in the same manner if the web is gently tickled with a straw, or if a fine jet of water is cautiously directed against it from a syringe or washing-bottle. We once saw a Harry Long-legs—as they are familiarly called—fall into a web, but manage to escape, leaving one of his ungainly limbs behind him. The spider, hurrying up and finding the foot, ran along, naturally expecting, doubtless, to find a body at the other end. Being disappointed she returned to the foot, and, when finally convinced that she had merely a trunkless leg to deal with, she fled with precipitation, as if utterly staggered at such a violation of the fitness of things.

The antennal language of ants—if it does not, according to Mr. Belt's interesting conjecture, turn upon the emission and recognition of odours—must depend upon touch. But if we look through the whole extent of what has been called "insect architecture" we meet with one unbroken series of instances, proving the utmost delicacy of touch in articulate animals.

If we try to discover the seat of the sense of touch in insects, we find several organs to which it has been assigned—the antennæ, the palpi, the paraglossæ, and even the feet may take a share in the process. It is, of course, probable that where so many organs exist there must be some variety in the nature of their functions. We may here be on the track of modifications of the faculty of touch or feeling which may be substantially to us unknown senses. Our power of touch refers mainly to solid bodies ; we can decide whether they are moist or dry, hot or cold, rough or smooth, &c., and we can discriminate between solids and liquids. But we have no organ that informs us of the state of the atmosphere, except in as far as we perceive its temperature by the whole surface of our bodies. It is far from improbable that some of the organs of insects may give them information of the condition of the atmosphere—baroscopic, hygroscopic, or electroscopic. This is the more likely since some of the parts which have been suggested as the seats of touch are, in multitudes of cases, exceedingly ill-adapted for being applied to the examination of solid bodies. The antennæ are often too short and too sparingly mobile.

It is plain that the more numerous and complete the data laid before us, the easier does the solution of any problem become. If, then, certain of the lower animals possess more delicate, and possibly more numerous senses, than do we, they are in a position to acquire, by direct perception, knowledge which we can only gain by trains of inductive reasoning and by the use of instruments of precision. Even yet Natural History is haunted by a phantom known as "Instinct," which is invoked in every case of difficulty, just as was phlogiston by the chemists of the last century, and which is invested, *pro re natâ*, with attributes not always the most conceivable or the most mutually consistent.* But very probably those instances of supposed instinct which are not resolvable into hereditary habit may be traced to the simple following of the guidance of senses more acute than our own. Thus we are told that certain birds, beasts, and insects have an instinctive foreknowledge of coming storms and of other meteorological changes ; that migratory birds leave us when the weather is still warm and sunny, and the insects upon which they feed are still plentiful ; that wild geese, fieldfares, stormcocks, &c., arrive earlier than usual on our shores, not because unusual cold has already set in at their ordinary resorts, but because it is going to do ; that the bees contract the inlet into their hive in proportion to the degree of cold which is about to prevail, &c. These observations, in so far as they are really founded in fact, are merely instances of warnings furnished by sensuous perceptions more acute than our own. We pronounce it a mystery that the swift should leave us by mid-August, when, according to our feelings, we are in the height of summer. But could we see and feel with the swift we might perceive a change amply sufficient to prompt migration.

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* Our great objection to "instinct" is that it is too often a word hiding ignorance under the veil of pretended knowledge. There are in Biology unsolved questions in abundance, some of which are possibly beyond the scope of the human intellect. But in such cases, instead of talking about "instinct," let us frankly confess that we do not know.

Even the attraction of a virgin female moth for the males of her species, as already described, has been referred to the convenient class of "instincts." It must be a curious instinct, indeed, which will act to the windward and not to the leeward. We never think of saying that the hound pursues the fox by instinct. If unwilling to concede to him the possession of reason, we may say that it is by instinct he knows that the odour he detects upon the grass or the soil has been left there by an animal which he may overtake if he follows up the trail. But the act of recognising this odour we ascribe simply to one of his senses. Why take another course as concerns the *Saturnia Carpini?* If an animal detects the presence of any object at a distance, it can only be effected by one of two methods; either material molecules, solid or gaseous, are given off by the object, and brought by atmospheric (or aqueous) currents into contact with the sense organs of the observer, or else certain vibrations or undulations, sonorous, luminous, &c., reach him through the medium of the atmosphere or the ether. Surely neither of these processes can be described as "instinct," and he who by implication admits the possibility of a third way should at least give us some hint concerning its nature and mode of action.

Here, therefore, is a point of departure for animal psychology which has not received a due share of attention. We must study the senses of the lower animals both structurally and functionally, more especially in the Articulata, where the departure from the human type of organisation is so complete, and where the manifestations of intelligence are so complex and so nearly rival our own.

II. THE SUPERFICIAL GRAVELS AND CLAYS AROUND FINCHLEY, EALING, AND BRENTFORD.

By THOMAS BELT, F.G.S.

I. *Introduction.*

I PURPOSE in the present paper to show the relation of the Glacial-beds at Finchley to the implement-bearing gravels at Ealing, and the mammaliferous gravels and sands at Brentford.

Possibly the objects I have in view—to demonstrate that the formation of the valley-deposits took place in the Glacial period, and that palæolithic man was pre-diluvial—might have been more clearly attained by a thorough study of one of the more northern valleys; but I have had much greater facilities for making myself acquainted with the district I have chosen. It has also this great advantage, that it is close to London, so that my descriptions and conclusions may be readily checked by an inspection of the numerous gravel- and clay-pits from which I have obtained the facts described in these pages.

II. *Description of the Deposits.*

1. *Finchley and Neighbourhood.*—The Glacial beds in the neighbourhood of Finchley were described in 1835 by Mr. Edward Spencer,* who traced the boulder-clay and underlying gravels from Muswell Hill to Finchley Common. Mr. Whitaker has mentioned them in his "Memoir on the Geology of Parts of Middlesex, &c.,"† and Mr. Henry Walker has described the beds exposed in the cuttings of the Great Northern Railway and the clay-pits in the vicinity.‡

My own observations date from July, 1875, when I first visited the clay-pits near Finchley under the guidance of Mr. J. J. B. Ives. Since that time I have neglected no opportunity of examining the numerous sections that have been exposed in excavations for the foundations of new

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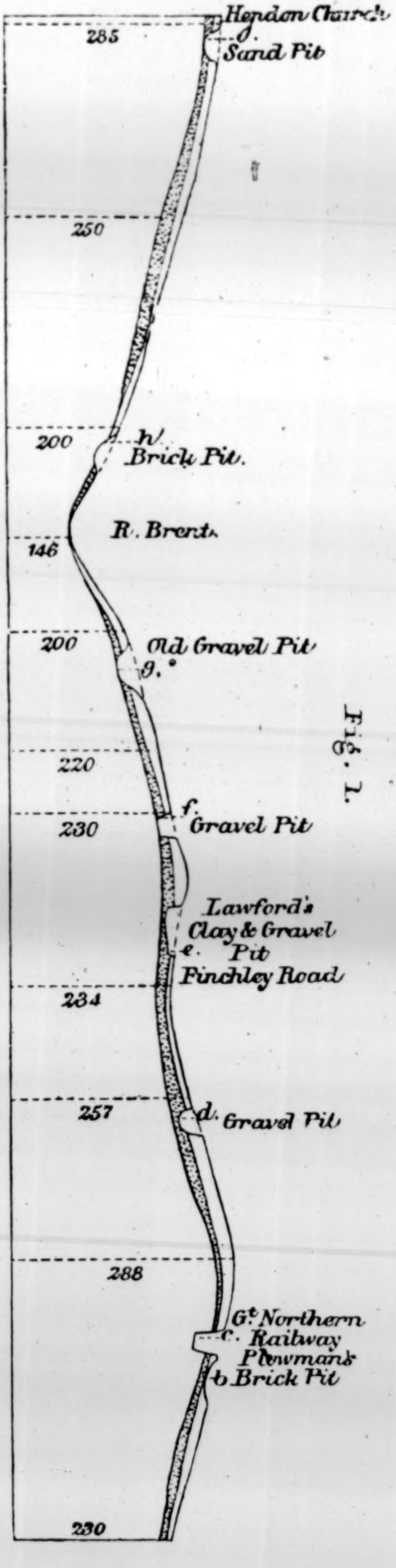
* Proc. Geol. Soc., vol. ii., p. 181.

† Mem. Geol. Survey, 1864.

‡ Proc. Geol. Assoc., vol. ii., p. 289, 1871.

Fig. 1.



SECTION ACROSS FINCHLEY TO HENDON.

Horizontal scale, 3 inches = 1 mile. Vertical scale, 1 inch = 200 feet. The figures express the height of the surface, in feet, above the Ordnance Datum-line. The dotted stratum is the Middle Sands and Gravels; the unshaded part above, the Upper Boulder-clay, as more particularly shown in the sectional details.

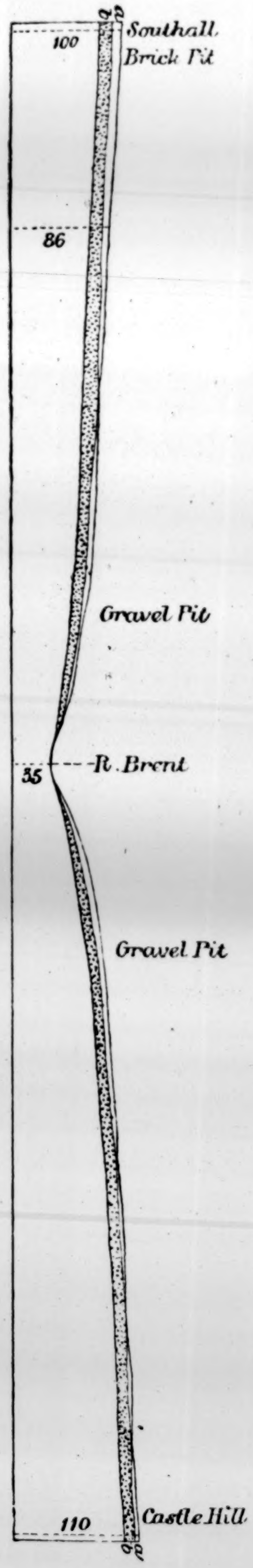


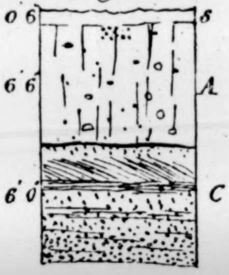
FIG. 2.—SECTION FROM ARGYLE ROAD, CASTLEHILL TO SOUTHALL.
Scale as above. *a*. Brick clays. *b*. Sands and gravels.

houses, as well as the more permanent ones in the gravel- and clay-pits. In this way I have been able to trace the beds right across from the drainage area of the River Lea into and across that of the Brent.

The general distribution of the deposits is shown in Fig. 1, which is a section extending from a little east of the "Green Man" public-house, on the St. Alban's Road, to the top of the hill at Hendon. The following are the details from which this section has been constructed:—

At an old brick-pit at the eastern extremity of my section the Upper Boulder-clay has been worked, but the surface is now grassed over. Lower down the slope I found a gravel-pit open about 250 yards directly west of the "Green Man" public-house, on the south bank of a small stream running to the Lea, and obtained the section shown in Fig. 3.

Fig. 3.

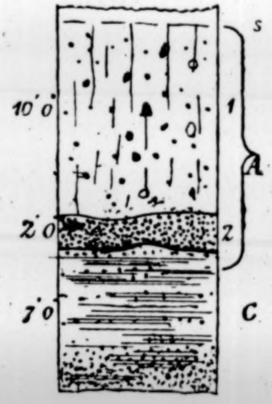


GRAVEL-PIT NEAR EASTERN END OF GENERAL SECTION.

s. Surface soil. A. Brown boulder-clay, unstratified, with bluish vertical partings. Stones mostly flints, with a few pieces of quartzite, sandstone, chalk, &c., the chalk mostly decomposed. C. Sands and gravel. Mostly sand at top, with lines of small gravel. Sandy coarse gravel below. Pebbles mostly subangular. Base not seen.

In this section the chalk has been mostly dissolved out of the clay, but pieces, generally quite soft, are still to be found in places, and in others the calcareous matter has entirely disappeared, leaving behind, nests of siliceous grains originally contained in the chalk. Ascending the hill westward, a good section of the boulder-clay is exposed in Mr. Plowman's brick-field. The clay here contains much chalk *detritus* and many other travelled stones, including pieces of granite, lias, and red chalk. Just beyond this is the cutting of the Great Northern Railway. The slopes are now mostly grassed over, but the beds were examined and described by Mr. Henry Walker when the section was well exposed. He found a chalky boulder-clay overlying a blue boulder-clay, with occasional patches of sandy gravel between them. At Finchley Station, about 550 yards to the north-west of the line of section, the chalky boulder-clay is exposed in patches overlying coarse gravel, and that again a sandy gravel. Further up the line this sandy gravel is superimposed upon a dark blue clay with much liassic *detritus.* When the cutting was being made Mr. Walker traced this blue clay for a distance of a mile and a half towards East End, considerably past my line of section. A little southwest of the railway the line of section crosses the top of the watershed between the Lea and the Brent. Descending the slope I was so fortunate as to find a large excavation for obtaining gravel and sand in the grounds of the Avenue, and the following section exposed :—

Fig. 4.



SECTION AT THE POINT MARKED *d* IN FIG. 1.

S. Surface soil. A, 1. Chalky boulder-clay. A, 2. Coarse gravel. Pebbles mostly rounded. C. Very sandy gravel. Mostly sand at top; coarser towards bottom of section. Base of gravel not seen.

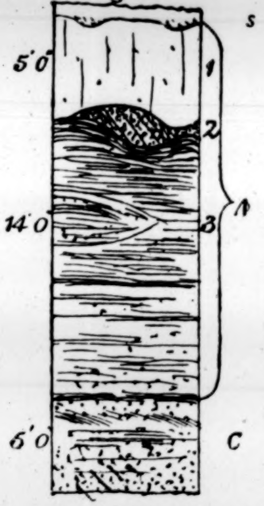
The upper part of the clay, both here and at Mr. Plowman's clay-pit, is brown and without chalk.

The line of section crosses a small valley running into the Brent, and, after passing the Finchley Road, reaches Mr. Lawford's brick-field at Church End. The beds exposed in this pit are shown in Fig. 5.

In this section the Upper Boulder-clay is much thinner than higher up the hill, and contains very few stones excepting in nests next the surface. These surface patches of pebbles mark the extent to which the clay has been subjected to subaërial denudation, the finer materials having been carried away and the coarser left behind. This denudation has been greater on the slopes than on the plateaux, so that it is on the latter that we find the clay now thickest.

The patches of gravel marked 2 in Fig. 5 are very irregular. Some of them look like lumps of the Middle Sands and Gravel that had been picked up and deposited in a frozen state, the originally horizontal stratification having in their new position been turned on end.

Fig. 5.

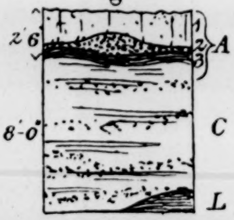


SECTION AT MR. LAWFORD'S CLAY-PIT, CHURCH END.

S. Surface soil. A, 1. Brown boulder-clay with few stones, excepting in nests near the surface. A, 2. Irregular patches of sandy gravel. A, 3. Alternations of laminated clay, sandy clay, and sand, with some lines of fine gravel. C. Sandy subangular gravel, with rounded quartz pebbles.

The line of section now crosses to Hendon Lane, where many gravel-pits have been opened, but most of them are now filled and houses built over them. In July, last year, I visited the spot, in the company of Mr. Henry Hicks, and obtained the section shown in Fig. 6, a little to the north of the line of the general section.

Fig. 6.



GRAVEL-PIT IN HENDON LANE.

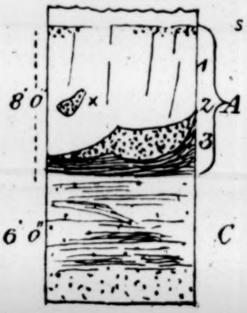
Brown boulder-clay, with few stones. A, 2. Irregular patches of gravel. 3. Laminated dark sandy clay. C. False bedded sands and small gravel. London Clay

The Middle Glacial Gravels here and throughout the Finchley district contain a considerable proportion of rounded pebbles from the Eocene beds.

Farther down the slope, in the field on the north side of Oldershill Lodge, the gravel has been largely worked. The following important section (Fig. 7) was exposed when I visited the pits in January last. The gravels extend to within about 40 feet of the level of the Brent, or Dollis Brook, as the stream is here named.

The upper clay A, 1, is here nearly without stones, excepting patches near the surface. It very closely resembles the Upper Brick-clay of the Thames Valley, and, as it is undoubtedly a continuation of the Upper Boulder-clay, and overlies the Middle Glacial Sands and Gravels, this change to the usual form of the brick-clay, as it descends into the valley, is very significant. In this section there was an angular piece of reddish sandy gravel, lying completely surrounded by the clay, at the point marked × in Fig. 7.

Fig. 7.



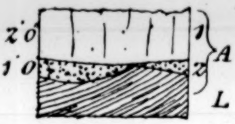
GRAVEL-PIT IN FIELD NEAR OLDERSHILL LODGE.

s. Surface soil. A, 1. Brown unstratified clay, with vertical joints. A few nests of pebbles at surface. Angular patch of gravel at ×. A, 2. Coarse gravel in sandy clay. A, 3. Dark, sandy, laminated clay. C. Sand and small sandy subangular gravel.

Descending Hendon Lane there are more old gravel- and sand-pits, in which exactly the same succession of beds is shown as in Fig. 7. These extend down to the line of 200 feet above the ordnance datum. Below this the ground slopes more rapidly towards the brook, and the middle sands and gravels thin out, but the Upper Clay, with its gravel patches at base, overlaps the Middle Sands and Gravels, and continues down to the bank of the brook, where it is seen as shown in Fig. 8.

Crossing now to the Hendon side of the stream, the surface deposits are shown in a large brick-field on the south side of Finchley Lane. The Upper Clay, with its accompanying patches of gravel, is thin, but it is seen to be continuous nearly to the brook. On the north side of the lane, along the line of section, it has been dug in several places; in others the clay has been denuded by natural agencies, and the coarse pebble gravel that lies at its base comes to the surface. The Middle Sands and Gravels come in below the clay at about the same height as on the Finchley side of the brook—that is, at about 200 feet above the Ordnance datum-line.* A little above that level, at the point marked *i* in general section (Fig. 1). I saw some sandy subangular gravel that had been thrown out in digging the foundation for a house. I have not seen anywhere on the Hendon slope these beds so strongly developed as they are on the opposite side of the brook, but Mr. Henry Hicks has informed me that near Heriot House they were 30 feet in thickness.

Fig. 8.



SECTION IN BANK OF BROOK BELOW THE HENDON LANE BRIDGE.

A, 1. Brown unstratified clay. A, 2. Coarse pebble gravel. L. London Clay.

Ascending the hill towards St. Mary's Church, the Upper Glacial Clay is seen in every road cutting and in excavations for new houses; but I did not see any deep enough to expose the sands or gravels until near the summit of the hill, where sand below the clay has been dug in several places, but the sections were not good enough for me to determine whether it belonged to the Middle Glacial beds or not.

On the west side of Hendon, down as far as the Midland Railway, the Upper Clay is everywhere present. To the south it is also seen wherever there are cuttings, but the Middle Sands and Gravels are not exposed, and I think they are mostly absent. To the northward the Chalky Boulder-clay is continuous from Finchley along the ridge past Whetstone. By the side of Church Lane, in Whetstone, the Middle Sands and Gravel are worked. I obtained here the section shown in Fig. 9. The surface of the ground is at this place about 290 feet above the Ordnance datum-line.

Mr. Caleb Evans has noticed the occurrence of the

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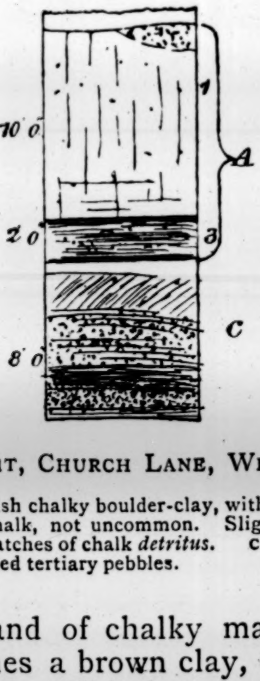
* The Ordnance datum-line is the mean level of the sea at Liverpool.

Glacial beds at Fortune Hill,* and on the slope to the north I saw both the Upper Clay and the Middle Sands and Gravels. To the east the beds extend as far as Muswell Hill.

Over the whole of this district the Upper Boulder-clay is spread, and conforms to the slopes of the hills, lying over them like a mantle. It contains most chalk fragments and other travelled materials on the flat-topped ridge on which Finchley is built, at a height of about 300 feet above the sea. On descending the slopes on either side the number of included stones and of chalky materials rapidly diminishes until it becomes a brown clay, containing only a few scattered pebbles. Mr. Whitaker records a great thickness of brown clay near Finchley Church, which, although it contains no boulders, he recognises as boulder-clay.† In the various sections I have examined, all the steps are to be seen in the gradation from a clay packed with travelled stones up to that in which only a pebble is to be found here and there. It is the non-recognition of the latter form of the deposit as a glacial clay that has led to the supposition that the Upper Boulder-clay is confined to the tops of the hills, and does not extend down their slopes.

Fig. 9.



GRAVEL-PIT, CHURCH LANE, WHETSTONE.

S. Surface soil. A, 1. Bluish chalky boulder-clay, with rather few scattered stones. Red and white hard chalk, not uncommon. Slightly stratified towards base. A, 3. Sandy loam, with patches of chalk *detritus*. C. Sand and sandy subangular gravel, with many rounded tertiary pebbles.

PHILOS. SOCIETY OF GLASGOW

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* Proc. Geol. Assoc., 1873, vol. iii., p. 30.

† Guide to the Geology of London, 1875, p. 55.

2. *Ealing and Neighbourhood.*—The superficial deposits around Ealing have been noticed by many observers. In the works of Prof. Prestwich and Mr. Whitaker are to be found many references to them. Colonel Lane Fox,* in 1872, gave an excellent detailed account of the gravels and clays around Acton, and showed in several sections the position of the flint implements and of the mammalian remains found in them. This memoir has been of great assistance to me in the study of the same deposits in the adjoining parish of Ealing. My own opportunities have been numerous for gaining a knowledge of their distribution, and of the nature and relation of their different component parts. In addition to the large gravel-pits which are always open, the small pits which are sunk at the building of every new house, for the purpose of obtaining sand and gravel, have supplied sections all over the district. In the cuttings made for widening the Great Western Railway unusually fine and continuous sections were exposed between Acton and Hanwell, or right across the parish of Ealing from east to west.

Westward from Ealing there are large gravel-pits on each side of the Brent, at Hanwell, and in making the foundations for widening the railway-bridge, sections of the lower part of the valley of the Brent at this point were exposed. Deep cuttings for sewers have given nearly continuous sections in a north and south line across Ealing; and, lastly, the construction of the new branch railway from Turnham Green to Ealing, now in progress, has given me opportunities for checking the results obtained in the other sections.

I made careful notes of all the sections at the time they were exposed, and soon found that there were certain dominant features that ran through the whole with remarkable persistency. Further study showed that these features ran parallel with those observed in the glacial beds at Finchley.

In the large gravel-pit at Castlehill Station the gravel comes to the surface at the western end, but at the eastern side it is covered with about 2 feet of unstratified brown clay, and presents the section shown in Fig. 10.

Both westward and southward the clay, *a*, 1, thins out, and the subsoil consists of the gravel, *a*, 3, and the flints that had been scattered through the clay before it was denuded. In the railway-cutting opposite the gravel-pit the gravel, *a*, 3, came to the surface. Going eastward it was seen to be gradually covered by the brown clay, and at about 100 yards east of Castlehill Station the section exhibited was as shown in Fig. 11.

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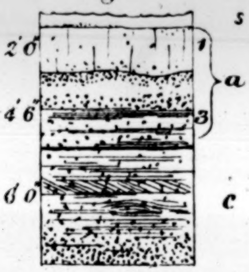
* Quart. Journ. Geol. Soc., vol. xxviii., p. 449.

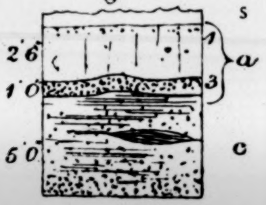
Fig. 10.



EAST END OF CASTLEHILL GRAVEL-PIT.

Surface soil. *a*, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Whitish gravel in clayey matrix, succeeded by alternations of ferruginous gravel and seams of sandy clay. c. Sand and very sandy gravel, sometimes false-bedded and with lenticular patches of sand in upper part. Base not seen.

Fig. 11.



RAILWAY-CUTTING 100 YARDS EAST OF CASTLEHILL STATION.

s. Surface soil. *a*, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Gravel in brown clay. c. Very sandy subangular gravel, with lenticular seams of yellow sand. Base not seen.

At Ealing Station a fine section was for a long time exposed, and I had the pleasure of showing it to Professors Morris and Bonney. It is represented in Fig. 12.

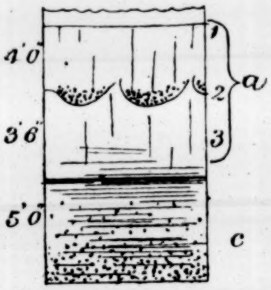
In it, the irregular patches of gravel *a*, 2 are more fully represented than is generally the case. Usually this division in the valley beds is only marked by a waved line of pebbles. At Ealing Station it consists of lenticular patches of mostly rounded pebbles lying on an extremely irregular surface of the clay below. They look as if they had been dropped whilst the beds below were so soft that the masses of gravel sank into them where they fell.

I might multiply instances and heap example on example to show the persistency and regularity of the beds, as I have great numbers of sections figured in my note-book, but no good purpose would be gained thereby, as everyone in the neighbourhood of London interested in the matter can observe them for himself.

The sections I have given, however, are all along an east and west line, with the surface of the ground a little over the 100-feet contour-line, and it will be well to describe the deposits at lower levels.

At Ealing to the south of the line of railway, the numerous sections shown in the small gravel-pits, dug when houses were building, all showed the same divisions. At about 90 feet above the Ordnance datum-line the brick clay is very thin, and sometimes absent through denudation; but below this line it again thickens. That it was originally present over the whole district is shown by the fact that patches of it are preserved all over the denuded areas in hollows or irregularities of the beds below. Thus in a long excavation that was made for a main sewer at Beaconsfield Villas, the trench ran north and south, and crossed two channels cut into the gravels. These channels were filled with the clay *a*, 1. I have represented one of these filled-up channels in Fig. 13. The other was about 80 yards to the north, and was deeper than that figured. The filled-up channels ran east and west.

Fig. 12.



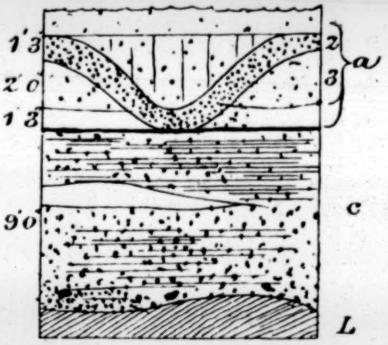
Railway-cutting, Ealing Station.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 2. Irregular patches of small pebbly gravel in clay. *a*, 3. Yellowish brown, rather sandy clay, a little stratified towards base. *b*. Sand passing downwards into sandy subangular gravel.

South of this the gravels continue down the slope, and at about 70 feet above the Ordnance datum-line are covered continuously with the clay down to near the 50-feet contour-line. At Ealing Cemetery, which is just above the 50-feet line, the gravel occurs in patches only, in hollows in the surface of the London Clay, and the brick-clay is thin, though still present. Below the 50-feet line the surface dips more rapidly, and on the steeper part of the slope the gravel is absent, but the clay, with its accompanying pebble-bed, though thin, is continuous.

Going westward from Ealing, towards the Brent at Hanwell, the beds down to the 50-feet line have been well exposed along the Uxbridge Road, in gravel-pits and other excavations. On the western side of the Brent the large gravel-pits opposite the Lunatic Asylum, and the Southall clay- and gravel-pits, afford good sections of the beds. The gravels descend both slopes of the valley, as shown in Fig. 2 (page 317). The lower gravel thins out at about the 50-feet contour-line, the upper beds overlapping it, and continuing nearly to the brook. Both conform to the slopes of the valley.

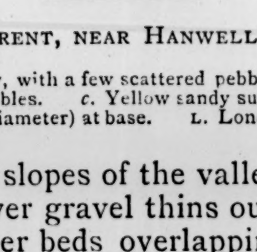
Fig. 13.



SECTION AT BEACONSFIELD VILLAS.

Surface soil. *a*, 1. Brown clay, with some pebbles. *a*, 2. Pebbly gravel in clay. *a*, 3. Irregular ferruginous sandy clay and gravel, with whitish stiff clay at base. *c*. Sandy gravel, with lenticular bands of yellow sand. Upper part of gravel irregularly stratified. Large flints and stones of quartz and quartzite at base. L. London Clay.

Fig. 14.

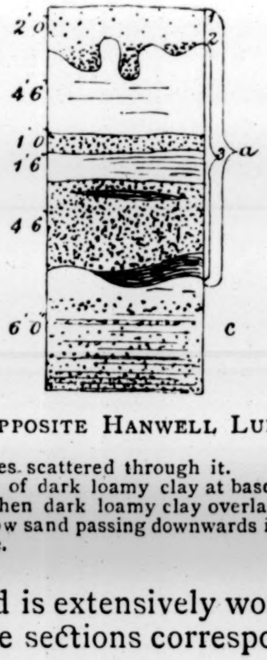


WEST SIDE OF BRENT, NEAR HANWELL RAILWAY BRIDGE.

a, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Gravel in clay, with many rounded pebbles. *c*. Yellow sandy subangular gravel, with a few large stones (12 inches diameter) at base. L. London Clay.

On the western side of the stream, a deep drain on the north side of the railway-embankment gives a continuous section down the lower slope of the valley-bank. Fig. 14 shows the section exposed, at about 20 feet above the Brent and about 50 feet above the Ordnance datum-line. Higher up the slope, at the large gravel-pit, the upper beds show more variety than is usual, the beds *a*, 3 being composed of a succession of clays and pebble-beds. Farther westward the clay *a*, 1 thickens, and is extensively worked for brick-making at Southall, where the sections correspond exactly with those at Ealing.

Fig. 15.



GRAVEL-PIT OPPOSITE HANWELL LUNATIC ASYLUM.

a, 1. Sandy clay, with pebbles scattered through it. *a*, 2. Pebbly gravel in clay. *a*, 3. An irregular seam of dark loamy clay at base; then a thick bed of pebble gravel in clay matrix; then dark loamy clay overlaid by pebble gravel, and that by sandy clay. *c*. Yellow sand passing downwards into sandy subangular gravel, with large stones at base.

The characteristics of the lower gravel are persistent throughout the whole district. It consists principally of angular and subangular pebbles of flint, with some rounded ones from the Eocene beds, and also pebbles and stones, generally rounded, of quartz, quartzite, Lydian stone, granite, and porphyry. It is extremely sandy, and the sand is distributed throughout it, and occurs also in lenticular patches. At its base it contains many large pebbles of quartzite, some a foot in length, and large unworn or slightly worn flints from the chalk. Large Sarsen-stones, 3 to 4 feet in diameter, are also found scattered over the district, at the base of the gravel.

The peculiar subangular character of the pebbles forming the bulk of the gravel, and the loose yellow sandy matrix, are the most characteristic features of the deposit. I had the pebbles in some samples of the gravel counted, and found that from 80 to 90 per cent were broken, or more or less angular. Thin chips of flint are abundant. Nearly all have the edges of the fractures a little rounded, as if, after they had been broken, they had been shaken together. About one-quarter of the rounded pebbles are of quartz.

There are slight and irregular signs of stratification in the lower part of the gravel in some instances; in others it is quite unstratified. Towards the top the stratification is more decided, but not continuous. Oblique lamination is frequent. It has the appearance of having been suddenly accumulated, and at one time. The irregular and fitful stratification, the short lenticular patches of sand, the mixture of sand throughout the gravel, the large stones at the base, the great proportion of broken flints with slightly worn angles, and the occasional oblique lamination, are all opposed to the theory that the deposit is the result of successive layers of materials brought down by a river at different times.

All over the district flint implements of the palæolithic type have been found in the lower gravel. According to the unanimous testimony of the workmen they occur nearly always amongst the larger pebbles at the base of the deposit, and close to the surface of the London Clay. Col. Lane Fox has described the position at which were found several flint implements at Acton, where the surface of the ground was from 75 to 83 feet above the Ordnance datum-line. Most of these were obtained from the base of the gravel, and the angles were worn and rounded. At one place, flakes were found in a thin seam of sand lying below the gravel, and in this instance the edges were " as sharp as when they were first flaked off the cores."* At Ealing Dean, Col. Lane Fox obtained two implements from gravel taken out in the construction of a sewer which was carried down below the general run of the excavations for the foundations of houses. According to this excellent authority they occur always at the base of the gravel. He says—" Here (in the lowest stratum of the gravel) the largest flint stones lie, and with them the implements, mostly of the dimensions of the larger stones, so that it was common for the more experienced workmen to say that they should find no implements till they got down into the coarse gravel; the smaller flakes, however, were not so invariably at the bottom."

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* Quart. Journ. Geol. Soc., vol. xxviii., p. 457.

Mr. Peter Crooke, of Turnham Green, has been very successful in finding implements in the Lower Gravel all over the Ealing district. One of these was obtained from the foundation of a house in Argyle Road, at the highest point the gravel reaches to, where the surface of the ground is about 120 feet above the Ordnance datum-line. Several were obtained from Grove Road, in Ealing, at about 80 feet above the same line, and many others in Gunnersbury Park a little above the 50-feet contour-line.

One fine specimen was found in Beaconsfield Villas, at the bottom of the gravel, a few yards south of the part represented in Fig. 13. The man that discovered it assured me that it lay directly on the surface of the London Clay. Mr. Crooke bears the same evidence to the position of the implements as Col. Lane Fox. He says that all the large ones come from the very base of the gravel. He has given me an interesting instance. He had for years watched the gravel-pit on the east side of the Brent, at Hanwell, but never could find an implement. Lately, however, the workmen dug down a little deeper than usual, and got down to the big pebbles at the base. Amongst the stones thrown out was a fine pointed implement, which is now in Mr. Crooke's collection. At Bollow Brook, near Acton, the gravel contained many fragments of wood in the same stratum in which some flint flakes were found. The only mammalian remains that have been found in the Ealing gravels are some rolled teeth of the mammoth; they occur at the base of the gravel, and are seldom met with.

The upper division of the superficial deposits that I have grouped together under the symbol *a* is made up of a series of beds differing greatly in composition. The lowest bed is often a seam of dark sandy loam, or of layers of ferruginous gravel, or of alternations of gravel with the dark loam. Very frequently it consists of gravel in a matrix of brown clay, and often there is a thin layer of sandy silt at the base.

Since the description of the sections has been written out I have found a buried forest bed in the cutting for the new railway from Turnham Green to Ealing, about 300 yards south of the east end of Ealing Common. The stumps of the trees are all small, the largest being 3 inches in diameter. They are rooted in the surface of the subangular gravel, and are all upright as they grew. The stumps are about a foot long, and buried in silt. They terminate upwards at the top of the silt, having apparently rotted off there. Above the silt there is from 2 to 4 feet of the brown clay, with patches of pebbles at its base.

The patches of gravel *a*, 2 are peculiar. They nearly always lie on an irregular surface of the beds below them, and look as if they had been dropped into the places they now occupy. Their place is often taken by an irregular line of pebbles.

The bed of clay marked *a*, 1 in the sections is the widespread deposit so extensively dug for brick-making. It is entirely unstratified. Pebbles are scattered here and there throughout it; in some parts they are scarce, in others numerous. These pebbles, by the partial denudation of the clay, are often collected into nests next the surface. The clay appears to have been originally distributed over the whole district, and to have been since partly removed by the action of the elements, the fine material of which it is composed and its position next the surface rendering it very liable to be washed away, especially on the slopes. When thin, it, with the irregular pebble bed at its base, forms the deposit named "trail" by Mr. Fisher. No organic remains nor implements have been found in the brick-clay in the Ealing district.

3. *Brentford and Neighbourhood.*—The gravels and clays at Brentford were brought before the notice of geologists in 1813, by Mr. W. K. Trimmer,* who described the occurrence of mammalian remains in the lowest division of the deposits. He stated that the uppermost beds were unfossiliferous, and that the bones occurred most plentifully at the base of the gravel, in hollows in the surface of the London Clay. In 1849 Prof. Morris† described a section exposed in the construction of a branch of the South Western Railway, at Brentford, near Kew Bridge. This locality was not far from one of the pits described by Mr. Trimmer, and Prof. Morris again notices the absence of bones and shells from the uppermost beds, and the great abundance of the bones at the base of the lower gravel. Col. Lane Fox, in the paper I have already mentioned, has described and given figures of the sections exposed between Acton Green and the Brentford Road, and has shown very clearly the position of the mammalian remains at and below the base of the gravel.

My own observations have been made in various gravel- and sand-pits in the neighbourhood of Brentford, Chiswick, and

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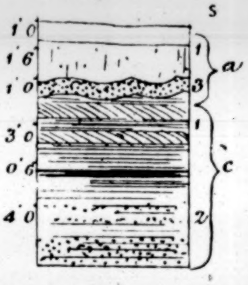
* Phil. Trans., 1813, p. 131.

† Quart. Journ. Geol. Soc., vol. vi., p. 201.

Turnham Green. Near the Kew Bridge Station of the South Western Railway the line has been widened, and a long section is exposed, showing a brown unstratified clay, with patches of pebbles at its base, overlying sand and sandy gravel. From the latter I obtained a small portion of a deer's horn. These beds were exposed in some sand-pits about 50 yards east of the station, as shown in Fig. 16.

Fig. 16.

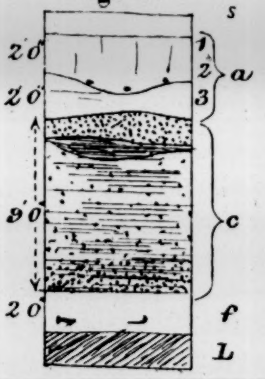


SAND-PITS NEAR KEW BRIDGE RAILWAY STATION.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 3. Gravel in brown clay. *c*, 1. Reddish, rather coarse, sand, with oblique stratification overlying a thin seam of dark loamy clay. *c*, 2. Yellow sand overlying very sandy subangular gravel.

I watched these pits for months, but could never find a fragment of a shell in them. A pit was sunk through the gravel to the London Clay below, for the foundation of a crane, and a seam of sand was met with at the base of the gravel from which several mammalian bones were obtained, now in the possession of Mr. T. Layton, of Kew Bridge.

Fig. 17.



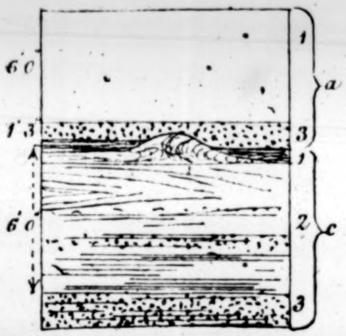
GRAVEL-PIT NEAR STYLE HALL.

s. Surface soil. *a*, 1. Brown unstratified clay. *a*, 2. Scattered pebbles along an undulating line. *a*, 3. Sandy clay. *c*. Ferruginous gravel at top, with lenticular patches of sand passing downwards into sandy subangular gravel, with a few stones of quartzite at base 7 inches diameter. *f*. Sharp sand, with bones. L. London Clay.

Nearer the river, close to Style Hall, I observed the section shown in Fig. 17.

Near to the place where this section was taken Mr. Crooke found a flint implement which had been thrown out from a depth of about 17 feet from the surface.

Fig. 18.

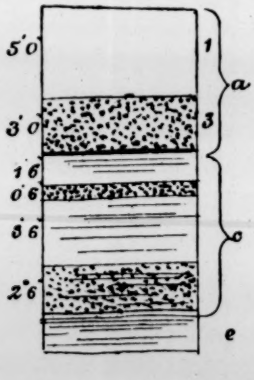


SECTION N.W. CORNER OF MR. TRIMMER'S OLD BRICK-FIELD.

a, 1. Brick-clay, removed by Mr. Trimmer. *a*, 3. Gravel in sandy clay, with some dark brown sand at base. *c*, 1. Curled yellow sand, with fragments of fresh-water shells. *c*, 2. Sand and sandy loam, with thin layers of clay, and some small angular gravel and minute fragments of shells. *c*, 3. Sandy subangular gravel.

Towards the western end of Brentford a very interesting series of pits have been opened to obtain sand and gravel. They are all on or near to the site of Mr. Trimmer's old western brick-field. Fig. 18 shows the beds exposed in a pit near the north-west corner of the old brick-field.

Fig. 19.



SECTION EAST END OF MR. TRIMMER'S OLD BRICK-FIELD.

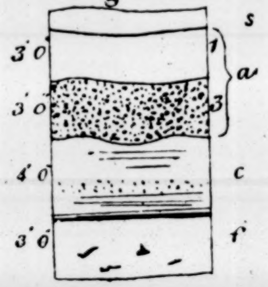
a, 1. Brick-clay, removed by Mr. Trimmer. *a*, 3. Gravel in clay. *c*. Layer of sand at top; then 6 inches of gravel in which was a broken valve of *Unio pictorum*; then sand and gravel. *e*. Yellow sand. *Unio pictorum* and *Cyclas rivicola*, common.

At the eastern end of the field the section shown in Fig. 19 was exposed.

About 100 yards north-east of the last section some pits have been sunk at various times, opposite Flora Villas, to obtain a peculiar sharp dark-yellow sand that is confined to a small area. The section exposed at one of these sand-pits is shown in Fig. 20.

There was water at the bottom of this pit. One of the workmen, at my request, pushed his spade down about 18 inches into the soft wet sand, and brought up from that depth a great number of shells, amongst which were specimens of *Unio littoralis* and *U. pictorum* with the two valves united. Many of the small cyclades also were perfect. This sand is full of shells, amongst which the little *Hydrobia marginata* is abundant. Neither *Unio littoralis* nor *Hydrobia marginata* now live in England. Their shells have been found in the Lower Thames brick-earths, but were not before this discovery known to occur so high up the valley as Brentford.

Fig. 20.



SECTION OPPOSITE FLORA VILLAS.

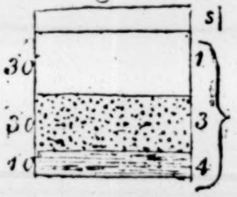
s. Surface soil. *a*, 1. Brown clay. *a*, 3. Gravel in clay. *c*. Coarse yellow sand, with a band of subangular gravel; a few broken shells in lower part. *f*. Sharp yellow sand, with mammalian remains and fresh-water shells, common. Base not reached.

The superposition of the subangular gravels to the shell-bearing sand was clear and decided. The workmen informed me that a stiff dark clay came in below the sand, and this is doubtless the London Clay. I obtained several mammalian bones from the same sand-bed, but they were soft and decomposed. The shells also were soft, and those of the *Unios* split up as they dried. Dr. Gwyn Jeffreys has kindly examined the shells from this pit, and has given me the following list of their names:—

Pisidium fontinale, var. *Henslowana*.
" *amnicum*.
Unio pictorum and *U. littoralis*.
Bythinia tentaculata and *B. Leachii*.
Hydrobia marginata.
Valvata piscinalis and *V. spirorbis*.
Planorbis complanatus and *P. nautilens*.
Limnea peregra and *L. truncatula*.
Succinea oblonga.
Helix pulchella.
Pupa marginata.

Of the above shells the little *Hydrobia marginata* was the most abundant, though, from its small size, easily overlooked.

Fig. 21.



SECTION 30 YARDS SOUTH OF SECTION SHOWN IN FIG. 20.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 3. Gravel in clay. *a*, 4. Laminated sandy clay or silt, with shells of *Helix pulchella*, *H. caperata*, and *Zua lubrica*.

In some other pits sunk near this the subangular gravel (*c* in section) was much more developed, and the underlying shell-bearing sand confined to a thin drifted seam. One of the sections (Fig. 21) exposed was noticeable as the only one at which I obtained any shells in the Brentford district from beds belonging to the upper division. They occurred in a thin seam of sandy silt, occupying the same position in the series as the silt enveloping the buried forest near Ealing, already described. The most abundant shell in the silt was the little *Helix pulchella*, and no aquatic species were found.

The gravels pass down below the Thames, and the bed of the river at Brentford is principally composed of reconstructed subangular gravel, with many shells of *Unio*, *Anodon*, *Nerita*, *Paludina*, *&c*.

I have the details of numerous other sections in my notes, all showing the same succession of beds, with the exception that I nowhere—excepting opposite Flora Villas—saw the bed of dark yellow sand with *Hydrobia marginata* and *Unio littoralis*. The bed of sand with mammalian remains, described by Col. Lane Fox as occurring at the base of the gravels in Brown's Orchard, may be the same, but he does not mention any fresh-water shells. As at that locality it also contained some rounded and angular pebbles, it was probably a slightly drifted bed mixed with the lower part of the sandy gravel. At Flora Villas the perfect shells of the species of *Unio* and *Pisidium*, with the two valves united, and the absence of all foreign materials, leads me to think that it was in place, and quite undisturbed since its original deposition.

I believe there are two distinct faunas represented in the lower beds, though the remains are often now found mixed together. The oldest of these two faunas is characterised by containing the remains of *Elephas antiquus*, *Rhinoceros hemitœchus*, *Hippopotamus major*, and *Cervus dama* var. *Clactoniensis*. It also contains the following species that occur in the succeeding formation:—*Equus caballus*, *Bos primigenius*, *Elephas primigenius*, *Cervus elephas*, *Ursus ferox priscus*, and *Felis leo*. This is the fauna that Col. Lane Fox found in the low-lying ground between Acton and Brentford. A single piece of the antler of a reindeer was found at the same place, but I think its presence may be explained by a slight mixture of the two faunas, as there are other signs of the partial reconstruction of the deposit.

The characteristic mammals of the second fauna are the woolly rhinoceros and the reindeer. Both faunas were originally contained in loose incoherent sands or in peat beds, and at the time of the violent outspread of the subangular gravels were more or less mixed together, and even caught up and distributed through the gravel. As they lived at the same time, one occupying a northern and the other a southern zone of country, they may sometimes have overlapped each other's range in their summer and winter migrations, as has been urged by Sir Charles Lyell and Prof. Boyd Dawkins.

The mammoth appears to have had a very wide range, and occurs in all the deposits from the Cromer forest-bed up to the diluvium, and is found associated, on the one hand, with such a southern form as *Rhinoceros etruscus*, and, on the other, with such northern forms as the reindeer and woolly rhinoceros.

The molluscan remains found in the Brentford beds favour the supposition of two faunas. In the lower beds occur *Unio littoralis* and *Hydrobia marginata*, both of which are southern mollusks not now found in England, and which lower down the Thames are found associated with the still more southern species, *Cyrena fluminalis.* In higher beds at Brentford, and mixed through the lower part of the gravel, I found a more robust form of *Unio pictorum* than occurs in the lower sands, along with a peculiarly thick form of *Cyclas rivicola*, but without any of the two southern shells. Prof. Morris, in the description of the beds near Kew Bridge, in which he found numerous remains of the reindeer, states that he could not find a fragment of *Cyrena fluminalis, Unio littoralis*, or *Hydrobia marginata,* although the northern species of mollusks were abundant.

The sandy subangular gravel lying above the mammaliferous sands is simply a continuation of that found at higher levels at Ealing. It only differs in containing more sand at the lower levels, and occasionally drifted shells and bones. The largest stones lie at the bottom of the gravel, as in the Ealing district: they consist of slightly worn flints, and more or less rounded stones of quartz, quartzite, and sandstone. At low levels—that is, below the 30-feet contour-line—bones of mammals are often found in this gravel, especially when, through the denudation of the older sands, it lies directly on the London Clay. These bones have been derived from the older beds. Mr. Trimmer states that the remains he found were most abundant at the base of the gravel, in hollows in the London Clay, where the deposit consisted of a heterogeneous mass of clay, sand, and gravel. In no instance, he states, have two bones been found together which were joined in the living animal. That the gravel is largely mixed with the pre-deposited sands is evident from the patches of the latter that occur in it full of the comminuted shells that are found uninjured in the undisturbed beds. Excepting these derived shells I have never seen any in the subangular gravel.

Passing on to the beds lying above the subangular gravel, we have again a most remarkable resemblance in the colour, composition, and succession of the deposits to those I have grouped under the same symbol in the Ealing district. In both cases the beds consist of unstratified brown clay, containing a few pebbles scattered throughout it and overlying patches of pebbles. Below the latter there are, occasionally preserved, remnants of a silty clay, in which I have found land shells near Brentford, and remains of a buried forest bed near Ealing, as already described. The brick-clay appears originally to have covered the whole district, but has been removed over much of the area from ancient brick-fields, the limits of which may still be traced by the abrupt slopes left at their boundaries.

III. *Mode of Formation of the Superficial Beds.*

From the time that the deposits at Finchley were described by Mr. Spencer, in 1835, they have been recognised as of Glacial age. On my first visit to Mr. Plowman's brick-field with Mr. Ives, we found pebbles of hard red and white chalk and granite, and specimens of *Gryphea incurva* in the Upper Boulder-clay. Various Liassic fossils, and fragments of granite, porphyry, micaceous sandstone, mountain limestone, coal, and oolite have been recorded from it. Mr. Searles Wood, jun., has recognised its identity with the Upper Boulder-clay of the eastern counties. He states that, even in South and Central Lincolnshire, and to the north of Gainsborough, "the material of the deposit is so identical with that on the brow of the Thames Valley that a basket of clay taken from either extremity of this area could not be distinguished, although these extremities are 140 miles apart." *

With regard to the origin of the Upper Boulder-clay there have been various theories propounded, but the majority of geologists seem to have come to the conclusion that it was spread out when the country was submerged and floating ice brought the stones contained in it from the north. Mr. James Geikie has indeed advanced the theory that the boulder-clay is the product of land-ice, and he would dispense with the action of floating ice altogether.† Even if some general arguments that have been urged against the theory of a mantle of clay having been spread over a country by the action of land-ice could be met, there are special objections to its application to the Finchley district. There, the boulder-clay caps the ridges between the valleys, and descends their slopes, lying like a cloak over loose beds of sand and gravel. Not a trace of the passage of Glacial ice is to be seen in these deposits, although the slopes are sufficiently great to have caused considerable movement of the ice down them if it had existed in the form of a glacier. The clay is thickest on the crests of the ridges, and contains there the most stones. Many of these stones have been transported from Yorkshire and Lincolnshire, and the ice that brought them, if it was land-ice, must have been sufficiently thick to cover all the low lands of Suffolk, Norfolk, and Essex, and pour over into the valley of the Thames through passes in the northern watershed, more than 300 feet above the sea. Yet from the neighbourhood of Ely, southward, not a trace of the passage of this immense mass of ice has been recorded in the eastern counties, although the boulder-clay generally overlies loose beds of sand and clay. That in some parts glacier ice might have passed over small areas under peculiar circumstances, and melted off again, without leaving any mark on the beds below, seems probable enough; but the distance from Ely to Finchley is about 60 miles in a direct line, and that a glacier of that length, and of sufficient thickness to over-ride the watershed of the Thames Valley, should have left behind it no trace of its passage, nor any of the moraines that mark the line of retreat of other glaciers, is a theory that cannot at present be accepted, notwithstanding the faith of its talented author in its efficiency.

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* Quart. Journ. Geol. Soc., vol. xxiii., p. 395.

† See Geol. Mag., 1878, page 73, for Mr. Geikie's latest views on this question.

With the theory of floating ice—by which is meant both that of coast ice forming during the winter, breaking up in the spring, and carrying away from the shores and distributing materials frozen to it, and that of icebergs breaking off from the ends of glaciers terminating in water—the phenomena seem to be in entire accordance. Local rocks are scarce; indeed nothing is more remarkable in the Upper Boulder-clay at Finchley than the absence of fragments from the underlying London Clay. The far-travelled stones are most abundant near the top of the ridge, where floating ice was likely to ground and deposit its freight. The stones are also scattered here and there through the deposit, as if dropped at different times during its formation. There are faint traces of lines of deposition in some of the exposures, and the general absence of stratification is not more marked than it is in the Upper Thames brick-clays, the formation of which is universally ascribed to deposition from water.

The much greater abundance of transported stones on and near the ridge of the hill, and the gradual diminution in their numbers and size on the lower slopes until the clay at the bottom of the valley contains only a very few scattered pebbles, is the very opposite to what we should have expected from the action of a glacier, and just what we should expect as the result of floating ice; for the former gravitates to the bottom of the valley, and deposits there a great proportion of the stones it carries along, but the latter generally in confined waters leaves its burdens on the shores. Around the lakes, and at the heads of the fjords of Nova Scotia, there are lines of great stones left by the ice when it breaks up in the spring. Sir Roderick Murchison, in his description of the distribution of the northern drift in Russia, shows that hills and slopes 200 or 300 feet high are often covered with large blocks, whilst the valleys between them, often wide, are free from them ; and again, that the northern rocks are scattered plentifully over the plateaux, and rare on the low plains, though not entirely absent.* Prof. James Hall has described similar facts with regard to the distribution of the northern drift in America.†

As the transported stones are most abundant at Finchley at a height of about 300 feet above the sea, we must suppose —if they were brought by floating ice—that the water at the time stood sufficiently above that level to permit of the flotation of the ice, and at the same time not too high to prevent it stranding on the ridge. The most of the stones have been brought from the north, from districts where we know there were glaciers, from the marks they have left behind them ; and it is probable, therefore, that the carriers of the transported stones were icebergs. If it had been shore ice that brought them we might have expected to find a preponderance of local stones from the hills in the vicinity, instead of which there is a great abundance of far-transported materials.

For icebergs, a depth of from 50 to 100 feet at least would be required for flotation, giving about 400 feet above the sea as a probable depth of water at the time they stranded on the hills at Finchley. That this really was about the height of the water at that time is probable, for if we colour a map of the drainage area of the Thames, so as to show the ground that would be then submerged, we shall find that we have opened avenues from the north-east connecting the Finchley deposits with wide areas in that direction that are covered with the same boulder-clay.

The Upper Boulder-clay overlies at Finchley, as it does farther north, sands and gravels with much oblique stratification, generally known amongst geologists as the Middle Glacial Sands and Gravels. The gravels contain many stones not found in place in the present drainage area. Patches of a boulder-clay, known as the Lower Boulder-clay, are found beneath them, from which these foreign materials seem to have been derived. We are thus led to conclude that there was an earlier submergence, during which the Lower Boulder-clay was spread out, and that afterwards it was subjected to some action by which it was greatly denuded and the materials derived from it re-arranged, the clay being washed away, and the sand and gravel now constituting the Middle Sands and Gravels left behind.

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* The Geology of Russia in Europe, pp. 510, 513, and 519.

† Natural History of New York, Part 4, p. 321.

The explanation I have to give of this series of events is the same that I have offered with regard to the formation ot the superficial deposits of Cornwall and Devon, of the loess of the Danube and the Rhine, and of the diluvium of the South of Russia: it is—that during the Glacial period a ridge of ice, starting from Greenland, gradually advanced down the bed of the Atlantic, and ultimately blocked up the drainage of Europe as far as it extended. A continental lake was thus formed, over which floated icebergs carrying the northern drift. The first lake thus formed was suddenly discharged by the breaking away of part of the ice-barrier. During the tumultuous outpouring of the pent-up water the materials deposited during the submergence, and others derived from the pre-glacial denudation of the surface rocks, were caught up and spread out in great sheets over the lower ground, thus forming the Middle Sands and Gravels. After a time the ice-barrier was re-formed, the country again submerged, and the Upper Boulder-clay spread out.

Passing on now to the consideration of the origin of the clays and gravels at Ealing and Brentford, the first noteworthy feature is the similarity in the succession of the beds to that of the glacial deposits at Finchley. This is brought out prominently in the figures by the nearly similar symbols under which I have grouped the divisions, but not more prominently than an inspection of the beds themselves will fully justify. The boulder-clay on the top of the ridge at Finchley, full of chalk fragments, is sufficiently distinct from the brick-clay at Ealing; but when we descend the slopes at Finchley the chalky boulder-clay gradually changes to a clay without chalk, until, in the lower parts of the valleys, it becomes a brown clay, with a pebble here and there, exactly similar to the brick-clays. The Middle Sands and Gravels at Finchley only differ from the subangular gravels of Ealing and Brentford in that the former contain a larger proportion of unbroken pebbles from the Eocene beds. This is only what we might expect on the theory of origin I have offered, for Finchley is nearer to the patches of Eocene gravels left on the hill-tops, and must have been largely made up from them. The greater distance the lower gravels had been carried, their exposure to the whole sweep of the torrents rushing down the Thames Valley, and their greater admixture with broken flints derived from the west, fully account

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for what small amount of difference exists between the contents of the two deposits.

I should, I believe, have little difficulty in establishing the position I have taken up,—that the gravels and clays of Ealing are simply the valley representatives of the glacial beds at Finchley, and that they were deposited at the same time,—if I had only the facts of the case themselves to deal with. I have, however, a much harder task before me; I have to contend against the general acceptance by geologists of Prof. Prestwich's theory,—that the valley clays and gravels are quite distinct from and newer than the drifts on the slopes and tops of the hills. I shall, in consequence, have to examine this theory in detail.

Briefly, Prof. Prestwich's celebrated theory is,—that after the distribution of the glacial drift, the valley of the Thames was gradually excavated, and the gravels and clays deposited during the process. It implies, therefore, that the whole of the Thames Valley, from the level up to which the gravels in question extend, has been cut out since the Glacial period.

The objections that may be urged against this view are many and serious. The gravels at Ealing reach to a height of 120 feet above the river, and the width of the Thames Valley at that level is more than 9 miles. If filled to that height there would be an expanse of water stretching from the northern side of Haven Green, at Ealing, to Morden, in Surrey, with some small islands consisting of the tops of the hills near Kingston, Richmond, Wimbledon, and Putney. A few miles farther west the valley is still wider, and without hills rising above the 120-feet line. All the valley below this line is supposed to have been excavated since the distribution of the glacial drift. Col. Lane Fox has shown us that there has been no appreciable change in the course or level of the river during the last two thousand years.* The theory therefore requires that we should put back the occurrence of the Glacial period many tens—and possibly many hundreds—of thousands of years, to allow time for the excavation of the valley from the 120-feet contour-line.

When, however, we go northward, to the region of undoubted ice-action, we find everywhere traces of the recent action of the glaciating agent, and that very little change has taken place in the configuration of the country since it was covered with ice. Thus Mr. Goodchild, in his description of the glacial phenomena of the North-west of England, states that "So little indeed has the aspect of the country changed in post-glacial times, that in many places the larger rivers are even now above the bases of the adjoining drift-mounds, whose present form can hardly be referred to any other than glacial action; and post-glacial denudation generally has effected so little that by far the greater part of the present surface-configuration has, in one way or another, resulted from the former presence of the great ice-sheet."*

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* Quart. Journ. Geol. Soc., vol. xxviii., p. 463.

Mr. D. Mackintosh, and I can quote no higher authority on the glacial beds of the West and North-west of England, in a recent lecture, at Chester, brought forward a great number of facts tending to show that the time which has elapsed since the close of the Glacial period need not have been so much as ten thousand years. Mr. Geikie and other geologists often refer to the recent appearance of the ice-markings in glaciated districts, and to the very small modification of the surface that has taken place since the outspread of the drift; but I need not refer more particularly to these opinions, as the neighbourhood of London itself affords sufficient evidence to the same effect.

At Finchley we see the Upper Boulder-clay mantling the hills and descending the slopes into the valleys, proving conclusively that the surface-features of that district, when it was spread out, were the same as they are now. The upper part of the valley of the Brent was then in existence, and at the most has not since been lowered more than 10 or 12 feet. The unconsolidated clay lies as it was deposited, and runs down to about 150 feet above the sea-level. It covers the hill continuously, and even on the steeper slopes has not been entirely denuded. Can it be for a moment considered possible that the great valley, several miles in width, has been worn out by denuding agencies below the level to which the boulder-clay reaches, and that above that line the action of the elements has been stayed, no appreciable change effected, and not even the uppermost bed of the glacial series denuded? How often must the river have changed its course, and travelled across from one side of the wide valley to the other, to remove the enormous mass that has been swept away in its excavation! Yet we are asked to believe that during all this time the denuding agencies did not operate on the uplands, but were stayed in their course, like the sun on Gibeon and the moon in the valley of Ajalon.

If we look at the country below the level of 150 feet above the sea, and compare it with that above it, we discover no alteration in the slopes, which blend the one into the other and are continuous, so that it is impossible to distinguish that horizon amongst the contours of the country. We might have expected that there would be some distinctly marked line, when, after the boulder-clay had been spread out, the river recommenced the excavation of the valley; but there is none. None, I mean, in Nature, though in the fanciful diagrams shown in most of our geological manuals the boulder-clay is represented capping the hills and presenting an escarpment to the valleys. If these sections were true ones, or even if there were one such in Nature, my opposition to Prof. Prestwich's theory would be greatly weakened, but I can find none, excepting in our books. In every valley I have examined, including that of the Ouse, at Bedford,—often appealed to in support of Prof. Prestwich's views,—the boulder-clay is not confined to the tops of the hills, but descends the slopes as it does at Finchley.

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* Quart. Journ. Geol. Soc., vol. xxxi., p. 99.

Prof. Ramsay and others have taught us that rivers excavate their channels from the sea backwards, somewhat in the same manner as the recession of the Falls of Niagara is effected. This is proved to be the case when, in new countries, the forest is cut down and valleys begin to be formed: they commence next the rivers and progress backwards, as Sir Charles Lyell witnessed in Georgia.* I have seen similar instances in Nicaragua and Southern Missouri. But on Prof. Prestwich's theory the upper parts of the valleys must have been excavated first and the lower afterwards—the tributaries of the river before the river itself; for it is evident that the valley of the Brent, at Finchley, was formed at the time the Upper Boulder-clay and the Middle Sands and Gravels were spread out.

From the bottom of the valley near Finchley, where the road to Hendon crosses the brook, to the line of my section shown in Fig. 2 (page 317), where it crosses the same stream at Hanwell, the distance down the valley—without following the minor sinuosities of the brook—is 9 miles. The boulder-clay comes down between Finchley and Hendon to 150 feet above the Ordnance datum-line; the valley gravels and clay at Hanwell rise to about 120 feet above the same line. The stream itself falls more than 100 feet in the 9 miles, or about 11 feet per mile. At the end of the distribution of the Upper Boulder-clay, if the lower part of the valley was not excavated, the stream would only fall 30 feet between the same points, or a little over 3 feet per mile, and yet, according to the theory of the post-glacial origin of the valley, have had power to spread out a continuous sheet of gravel, without at the same time being able to wash away the loose Middle Sands and Gravels with their thin covering of clay on the slopes at Finchley.

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* Principles of Geology, ninth edition, p. 205.

These seem to me formidable preliminary and general objections to the theory; but let us suppose that they have been overcome or explained, and try to follow in our mind what would happen if the valley below the 150-feet contour-line had been left, at the end of the Glacial period, an undulating plain, covered more or less with boulder-clay and underlying sands and gravels, through which the streams began to cut their channels. We have no great body of water to do this in the valley of the Brent below Finchley, if between Finchley and Hendon the stream was not powerful enough to wash away the glacial beds. The drainage area of the Brent is also too small a one to have supported a large stream. To excavate the wide valley from below the "Welsh Harp" to the Thames, it must therefore have often changed its course and wandered from side to side, so as to operate at different points. It would first have to clear away the glacial beds, and then cut down into the London Clay. We find, however, no bluffs in the latter such as we might expect, but a long gentle slope, from Ealing down to the stream at Hanwell, covered continuously with a mantle of gravel and clay. The superficial beds could not be deposited without break or overlap during the excavation of the valley, but only after the escarpments, formed whilst it was being cut out by the stream, had been sloped down by subaërial denudation. Every stream that is lowering its bed leaves precipitous banks, but the sides of the valleys of the Thames and its tributaries had been bevelled off into gentle slopes before the deposition of the gravels.

We may next inquire if the gravels and clays in question resemble those that are now being deposited by streams. When travelling in Russia I examined the sands and gravels of the beds of the large rivers. These rivers are frozen over for several months every year, and it seemed likely that the conditions would be somewhat similar to those that are supposed to have existed in the valley of the Thames at the close of the Glacial period. Much of the sand and gravel from the rivers of Southern Russia was used for ballasting the railways, so that I had many opportunities of examining it. I found it in every case full of river-shells, and sometimes one-half of the whole mass seemed to be shells. There were three species of *Unio* and the little *Neritina fluviatilis* especially abundant. When gravel from the bed of the Thames is now dredged up, it also contains many shells. In the sections exposed in 1875, during the extension of the embankment west of the Houses of Parliament, the bed of the river was seen to be crowded with shells, principally *Unio pictorum* and *Bithynia tentaculata.* Now, it is another objection to Prof. Prestwich's theory that the gravels that he supposes were deposited in the old river do not contain river-shells. I never saw even the fragment of a recent species of river mollusk in all the sections I have examined around Ealing, and none have been recorded by others. At Brentford, when we get down nearly to the level of the present river, drifted shells are often met with in the gravel, but they have evidently been obtained from the older sands of the pre-diluvial river, and do not belong to the time of the deposition of the gravels.

The brick-clay that overlies the gravel is considered by Prof. Prestwich to be inundation mud. We have silty deposits now forming or lately formed on the low flats adjoining the Thames: they consist of dark blue clay, with remains of vegetation and land and fresh-water shells. Sometimes peat beds occur. The brick-clay, on the other hand, is a homogeneous unstratified brown clay, without any organic remains. We have thus the formation of two deposits, the valley gravels and the brick-clays, ascribed to river action, and yet, in all the sections exposed, differing in most important particulars from the sediments now being deposited by rivers.

The absence of these remains from some of the beds might be explained, but it seems impossible to believe that the excavation of the valley could have progressed during several thousands of years, and the river everywhere have left behind it thick deposits of gravel and clay, without any river shells. The preservation of the fragmentary shells at Brentford, derived from the older sands, proves that their absence is not due to their destruction since the outspread of the gravels.

The structure of the deposits is opposed to the origin claimed for them. A continuous sheet of gravel covered by another of clay, spread over the slopes of a valley, is not what we should have expected. The river, in wandering from side to side of the wide valley it was wearing down, should often have cut into and truncated the deposits it had left at a higher level, and could not have exactly joined on its later sediments to its earlier ones. Nor does river action explain the angular and subangular character of the gravel, nor the presence of the largest stones at the base of the deposit.

If, then, the physical evidence is so opposed to the theory, why should it be upheld? Only, so far as I can understand, because the conclusion is started with that palæolithic man is post-glacial, and only by the theory of the post-glacial excavation of the southern valleys can the post-glacial age of palæolithic man be defended. But why should we commence with an assumption like this? At Bedford the evidence is a repetition of that which we have in the Thames Valley, and I have shown in a former paper that at Hoxne* there is no proof of the post-glacial age of palæolithic man. The palæontological evidence is also hostile to Prof. Prestwich's theory. At low levels in the Lower Thames Brick-earths the remains of *Rhinoceros megarhinus*, *R. hemitæchus*, and *Elephas antiquus* occur, all of which belong to the oldest pleistocene fauna of Europe. At much higher levels the stone-implements of palæolithic man are found, along with the remains of the mammoth and the woolly rhinoceros. On the supposition that the remains have been entombed during the gradual excavation of the valley, the latter would be the oldest of the two faunas. The great mammals, the bones of which are found so plentifully at Brentford, would have lived long after the men who left their chipped flints at the much higher levels at Ealing. The affinities of the fauna found in the Lower Thames Brick-earths with that of the Cromer forest-bed are much closer than that found in the gravels. And in the valley of the Rhine the deposits, with the woolly rhinoceros and the mammoth, overlie those containing *Elephas antiquus*, whilst they ought, if Prof. Prestwich's theory was correct, to underlie them.

Mr. Alfred Tylor has propounded the theory that the gravels were spread out after the excavation of the valleys to their present depth. He supposes that the Glacial period was succeeded by a Pluvial period marked by an enormous rainfall, so that the river-floods reached far above their present limits, and deposited gravels and clays high up the slopes of the valleys. The highest beds may, he thinks, have been formed by rain-wash. He has also suggested that the gorging by ice of the mouths of the Somme, the

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* Quart. Journ. of Science, July, 1876.

Seine, and the Thames, may have assisted in the production of some of the gravel-beds.*

I am so much indebted to the suggestive papers on the superficial deposits, by Mr. Tylor, that I regret that I cannot accept his theory as satisfactory. The gravels and clays of the lower slopes do not differ from those of the higher ones, and it is not probable that rain-wash would produce the same effects as a flooded river. The fact that the glacial deposits at Finchley remain intact nearly to the level of the present brooks shows that they have not been exposed to the action of any extraordinary denuding agents, and floods and rain-wash that did not remove them could not spread out great sheets of gravel lower down the valley. The gorging of rivers by ice is doubtless a true cause of great floods, but water so dammed back could not reach to the levels we require, and would only deposit silt, and not spread out gravels. The theory also offers no explanation of the close similarity in the succession of the glacial and the valley deposits, nor of the resemblance of the Middle Glacial Sands and Gravels to the subangular gravels of the valleys.

The theory that I offer in the place of these is in complete harmony with both the physical and the palæontological evidence. It asks for no other agency than that concerned in the outspread of the glacial beds, and explains the similarity of those to the valley deposits by recognising in the latter the representatives of the glacial clays and gravels of the hills. It offers no opposition to the evidence of the comparatively short time that has elapsed since the Glacial period, as it admits the pre-glacial age of the southern valleys as well as that of the northern ones. It does not require us to believe that in the one case the configuration of the country has been completely changed, whilst in the other it has remained unaltered. It affords an explanation of the disappearance of many large animals that formerly abounded in Northern and Central Europe, and accounts for the multitude of their remains at a particular horizon. A new light is thrown by it on the cause of the present distribution of animals and plants. The strongly-marked break that exists between the times of palæolithic and neolithic man finds in it a solution, and the traditions of a great deluge a foundation.

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* Quart. Journ. Geol. Soc., vol. xxv., p. 10, and Geol. Mag., 1875, p. 437.

TABULAR VIEW OF THE RELATION OF THE SUPERFICIAL DEPOSITS OF THE THAMES VALLEY TO THE GLACIAL PERIOD.

F. LOWER BRICK-EARTHS. OLDER MAMMALIAN FAUNA	Atlantic ice reaches the North-west coast of Scotland. Straits of Dover cut through by the drainage being turned southward.
E. NEWER MAMMALIAN FAUNA	Atlantic ice progressing slowly southward. River-beds lowered through the depression of the sea-level.
D. LOWER DILUVIUM	Atlantic ice reaches the Western end of the Pyrenees. First European Lake, and great destruction of Mammalian life.
C. MIDDLE SANDS AND GRAVELS	Break in the ice-barrier. Sudden and torrential discharge of the water of the European Lake.
B. LAND SURFACE	Gap in the ice-barrier still open.
A. UPPER DILUVIUM	Gap in ice-barrier closed up again. Second European Lake gradually lowered by the cutting through of a channel between the Black Sea and the Mediterranean.

I have described the main features of my theory in several papers published in this Journal and in the "Journal of the Geological Society, and I shall now seek to apply it more particularly to the deposits described in this paper. To render the subject as clear as possible I have drawn up the preceding tabular statement, showing the relation that the deposits of the Thames Valley bear to the Glacial period.

F. *Lower Brick-Earths. Older Mammalian Fauna.*—The Lower Brick-Earths are only represented in the Brentford district by the small undisturbed patch that I found opposite Flora Villas, and by more or less drifted remains of the older mammalian fauna. They are more fully developed in the lower part of the Thames Valley at Erith, Crayford, Ilford, and Grays Thurrock. The depositions of these brick-earths in the south-eastern counties was preceded by a land surface represented in the rootlet beds of Kessingland, Hasborough, and Runton.

The fauna found in the deposits is closely related to the still older Cromer Forest-bed, by the occurrence of *Elephas priscus*, *E. antiquus*, *Rhinoceros megarhinus*, *R. hemitæchus*, and *Cervis dama*, var. *Clactoniensis*, amongst the mammals, and *C. fluminalis* and *Hydrobia marginata* amongst the mollusks. *R. etruscus*, another Cromer Forest species, has been found in beds, apparently of the same age as our Lower-Brick earths, in the valley of the Rhine, along with *Elephas antiquus*.

In the Thames Valley there are few deposits of this age that were not more or less affected by the cataclysm that spread out the middle sands and gravels. The small patch found opposite Flora Villas at Brentford, some of the lowest beds in the Lea Valley at Clapton, and the Grays Thurrock brick-earths are the only instances I know of where the sands appear to be quite undisturbed. The mammalian beds at Ilford, Crayford, and Erith are, I think, all slightly drifted, and contain some admixture of remains belonging to a later date.

Before the Atlantic ice reached the north-west coast of Scotland, the Straits of Dover do not appear to have been cut through, and the German Ocean only communicated with the Atlantic northward. By the arrival of the ice from the north-westward on the coast of Scotland, the water of the German Ocean area was dammed back, and rose until it flowed across the neck of land joining England to the Continent. A lake was thus formed draining to the southward, which was gradually lowered by the cutting through of the Straits of Dover.

This was probably the time of the greatest range of the Hippopotamus which ascended the great river to the Germanic lake. Possibly the river, by the lowering of the sea level, flowed as far southward as the Bay of Biscay, and if, as Dr. Gwyn Jeffreys supposes, the Mediterranean then communicated with the Atlantic to the north of the Pyrenees, the hippopotamus would not have to travel far from the Mediterranean area, where we know it abounded, to the mouth of the great river.

Palæolithic man appears to have penetrated into Britain at this stage, as his implements are found associated with the older mammalian fauna in the caves of the west.

E. *Newer Mammalian Fauna.*—The last stage gradually merged into the present one by the climate—affected by the continued advance of the northern ice—becoming too cold for the southern fauna, which retired further south; its place being taken by the mammoth, the woolly rhinoceros, and the reindeer. The great ox, the wild horse, the bison, the red deer, the Irish deer, the lion, the bears, and the hyænas appear to have lasted on from the former period, and the three first to have abounded. The hippopotamus still occasionally came up the great river in the early part of the period. The mammoth, which during the time of the deposition of the brick-earths had reached as far south as the Thames in its winter migrations, now became a more permanent resident. The most characteristic mammals are the woolly rhinoceros and the reindeer, though even they, in the early part of the period, must have overlapped in their winter migrations the range of the more southern animals in their summer wanderings. The reindeer is absent from the Lower Brick-earths, and from the beds containing the older fauna at Brentford, excepting a small portion of an antler found by Colonel Lane Fox, which may have been drifted at the time of the debacle, when the remains of the two faunas were partially mixed together. In the section at Brentford, described by Prof. Morris, the reindeer was abundant, and was associated with the mammoth and woolly rhinoceros. Not a fragment of the southern shells, *Hydrobia marginata, Unio littoralis,* and *Cyrena fluminalis,* occurred at this spot, whilst in the older sands in the same district the two first-named are abundant.

Mr. Godwin Austen has described peat and sedimentary beds of this age underlying the gravels at Pease Marsh, near Guildford.* In these the nearly perfect skeleton of a mammoth was found. Remains of trees occurred, rooted in the underlying Neocomian clay, the whole being overlaid by the subangular valley gravels.

To this period belongs most of the flint implements of the Thames Valley gravels. When found in the gravels they are mostly worn by rolling, but at Acton in the deposit, described by Colonel Lane Fox, below the gravel they were quite unrolled just as Mr. Godwin Austen found the uninjured bones of the mammoth in a similar situation at Pease Marsh.

The localities where the flint-implements occur in most abundance are just such as might have been chosen by palæolithic man if the configuration of the country was then the same as it is now. Perhaps the most conspicuous instance of this is the Milford Hill Station near Salisbury. Milford Hill is described by Dr. Blackmore as forming a buttress between the two valleys of the Bourne and the Avon above their junction. It is separated from the main tract of high land behind by a transverse valley, thirty feet in depth, so that it forms an isolated hill.† Such a position was an admirable one for a rude tribe dependent for their living on hunting and fishing; giving them the command of two valleys, and being easily defended from attack. In our own district it is noticeable that the implements are found in greatest abundance in situations presenting somewhat similar features. Thus at and near Mill Hill at Acton numerous flint-implements have been discovered in the lower part of the gravel. Mill Hill lies between Acton Brook and Bollow Brook, and to the south, slopes rapidly down towards the low flat bordering the Thames. The locality in Gunnersbury Park, where Mr. Crooke obtained many implements, is on the crest of a similar slope, and immediately to the east of another small brook. In such situations palæolithic man might easily secure himself from the great wild beasts that frequented the lower ground near the river; he held a position of defence with regard to other tribes, and the brooks in the vicinity would probably be frequented by many of the smaller animals the objects of his chase. Mr. Crooke found numerous semi-fossilised pieces of wood along with flint implements in the gravel on the east side of Bollow Brook, and it seems not unlikely that these were the remains of a stockade that had surrounded the old palæolithic station.

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* Quart. Journ. Geol. Soc., vol. xi., p. 112.

† *Ibid.*, vol. xxi., p. 250.

The significance of these facts is entirely lost if we have to suppose that the configuration of the prediluvial land was not the same as it is now.

D. *Lower Diluvium.*—During the last stage I suppose that the Atlantic ice had slowly progressed southward, the climate of the temperate zone becoming every year more vigorous in consequence. In those last days of palæolithic man the reindeer lived the whole year through in Southern France, and the musk sheep reached in its migrations the same low latitude.

The readers of this journal who have followed me in my former papers will recollect that I have already given some explanation of the cause of the accumulation and of the mode of advance of the Atlantic ice. I feel, however, that I have not dealt with this important phase of the question with the fulness it deserves as lying at the very foundation of the theory. I must ask my readers to bear with me whilst I speak of this advance of the Atlantic ice as a fact, fully recognising my obligation in so doing to address myself at the first opportunity to the solution of the physical difficulties of the problem. They evidently weigh heavily with many scientific authorities without being actually defined. They are indeed more fanciful than real, more a failure of the imagination to conceive a state of things so different from that now existing than any clearly perceived objection; but reason has a stronger pinion and a more piercing eye than imagination, and leads the way to regions that would remain unknown to her sister excepting for her guidance.

I suppose that the Atlantic ice at last reached the Pyrenees, or coalesced with the ice flowing from that mountain-chain, closing the outlet of the northern drainage of the Continent to the Atlantic. The gap between the western Alps and the eastern Pyrenees was also filled with ice, and Behring's Straits similarly closed. The communication between the Black Sea and the Mediterranean not having yet been cut through, the waters began to rise over all the northern parts of Asia and Europe. The bulky mammoths and rhinoceroses frequenting the low plains would probably be the first to be overwhelmed. Some might find a temporary refuge on low isolated hills only to be overtaken by the rising water. The higher ranges of hills would form the chief places of refuge, but many of these, especially in western Europe, were covered with ice. The Altai mountains in Central Asia, the Urals, the Caucasus, the Carpathians, and the mountains of Armenia were likely the ranges on which many animals and plants found security, and which formed centres of dispersion in post-diluvial times. The ethnologist now finds on some of these mountains remnants of diverse peoples, and the naturalist animals and plants preserved there and absent from intermediate tracts of country.

The non-occurrence of human bones along with the flint-implements and the remains of the great mammals in the British Isles suggests that the palæolithic people of these parts escaped, at least for a time, from the rising waters. Their numbers may have been few, and they may have lingered for a time, and more gradually have died out on the hill tops to which they fled, or they may have taken to canoes and rafts and tried to reach more southern shores across the great waste of water that surrounded them.

Some of the palæolithic people of Central Europe may have escaped into Italy, but that country was probably inhabited by a different and a hostile race, the ancestors of neolithic man who spread over central Europe when the great floods abated; finding the land covered with a fertile soil, freed from the great carnivores and without inhabitants to contest their occupation of the country. Possibly the ancestors of the Basques lived to the south of the Pyrenees in the Glacial epoch, and spread in like manner into Southern France in early post-diluvial times.

C. *Middle Sands and Gravels.*—The Middle Sands and Gravels were spread out, according to this theory, by the sudden and torrential discharge of the pent-up waters, caused by the breaking away of the ice dam between the Pyrenees and the Alps. The course of the flood across the south of England was from the north until the water was lowered, and the rushing torrents confined to the valleys, the direction of which they then followed. In the Thames Valley, as soon as the height of the water fell to the level of the water-shed to the north, the rush would be down the valley or from the west; though until it was lowered to below 200 feet above the sea, some of the flood would escape southward through the low pass between the Wey and the Arun.

The materials that the flood would find on the surface, in a loose and unconsolidated state, would be varied and numerous. Over all the chalk area there would be the flints left during the long subaërial denudation to which the chalk had been subjected in pre-glacial times. There would be the patches of Bagshot sands, only small remnants of which are now left on some of the hills, and other tertiary sands and gravels where they came to the surface. To these local materials would be added all those brought by floating ice before the first lake was discharged, and constituting the Lower Boulder-clay.

These materials would be caught up by the mighty flood, swept away, mingled together, and ultimately spread out over the lower lands, when the violence of the debacle abated. If it had not been checked it would have probably carried all its spoil into the sea. The lessening of the fury of the flood, when it reached a level of about 200 feet—and still more below 100 feet—above the sea, must have been due to the outlet of the water being contracted or obstructed. The channel across Languedoc to the Mediterranean may not have been broad enough for the rapid discharge of the great lake below 200 feet above the sea. Whatever was the cause, the evidence shows that the violence of the flood was checked, though not entirely stopped, and the materials that had been swept off from the upper slopes were spread out over the lower ground.

At Finchley we find the Middle Sands and Gravels, and even patches of the Lower Boulder-clay, at heights from which they are entirely absent on the flanks of the hills bordering the Thames Valley, as, for instance, the southern slopes of Harrow and Hampstead Hills. The preservation of these deposits at Finchley is due to the fact that to the north and north-west there is a ridge of high land rising to over 400 feet above the sea, which would protect them from the violence of the flood when it was moving from the northward; and when the water was lowered to below 400 feet the current would be from the westward, and Finchley would be completely sheltered from it by the high chalk ridge running south-westward from Tring. The southern slopes of Harrow and Hampstead would, on the other hand, be exposed to the full violence of the torrent rushing down the Thames Valley, and if, as is likely, gravels had been deposited on them whilst the current was from the north, they must have been utterly washed away later on when it was from the west.

Below 200 feet above the sea, when the violence of the flood was somewhat checked, the outspread of the materials it was hurrying along began; at first in sheltered places; then, as the water was still further lowered and its velocity still more lessened, more generally and continuously.

The level at which these deposits began to be the rule, instead of the exception, was at about 120 feet above the Ordnance datum-line. Below this level they were spread out over all the flat or gently sloping ground, and only on the steeper gradients did not find a lodgment. As the water still further fell it drained into the old pre-diluvial smaller valleys, and in some cases the gravels have been swept out of these for some distance above the levels of the present brooks. The gravel and sand being all in motion together, there was a rough sorting of the materials, according to their weight; the largest and heaviest stones found their way to the bottom, whilst the sand was more generally distributed near and at the top of the gravel.

Pre-diluvial man had left his stone-implements around his old settlements, and many of these were mixed up with the materials brought down from the west by the flood and deposited with them, the larger flint-implements sinking to the bottom along with stones of the same size. At still lower levels many of the implements were not moved far from where they had been left on the surface, and in some instances were not moved at all. Above the 40-feet contour-line the bones of the large mammals that may have been lying on the surface deposits before the debacle, have been mostly swept away, excepting where preserved in hollows from the violence of the flood. A few of the large heavy teeth of the mammoth have been found in the gravels of the Ealing district, but the lighter bones are absent. In 1875, in digging gravel at the site of the New Museum of Natural History, at Kensington, the tooth of a mammoth was found at the base of the gravel, within 6 inches of the surface of the London Clay. No other bones occurred with it. A little farther west, in digging the foundations for houses in Cromwell Road, many fragments of bones belonging to the great ox, the red deer, and the mammoth were found near the top of the gravel, but none of the heavier teeth. It would appear from this that the remains were, like the stones of the gravel, deposited according to their specific gravity.

Below the 40-feet contour-line, in the Brentford district, mammalian remains are most abundant. The reason why they should have been preserved there, is apparent when we look at a map showing the contour-lines of the district. To the west of Brentford, a spur of high land, all above 70 feet above the sea-level, comes down from Southall, as far as Hounslow. To the east of this the 50-feet contour-line runs to the north from Brentford, up past East Acton, towards Wormwood Scrubs, forming an inlet protected from the violence of the western flood. It is in this sheltered bay that the mammalian remains, so abundant between East Acton and Brentford, have been preserved. Mr. Alfred Tylor, some years ago, drew attention to the fact that all the mammaliferous deposits of the Thames Valley occur in situations similarly protected by escarpments to the westward; and my experience has been to the same effect.

We have thus, on the one hand, deposits completely broken up by the violence of the flood, and, on the other, completely preserved from it. All the gradations between these two extremes should and do exist. In some the remains have been broken and mixed through the gravel, and even sorted according to their specific gravity. In others they have been drifted for a short distance, and the two mammalian faunas are mixed together without the bones being broken or much rolled. In others they have been only a little drifted from their original position; and in others, again, remain as they were first deposited, the bones of the same animal lying together.

Much confusion must thus arise, and much difficulty in determining the succession of the beds. The difficulty is often increased when the remains lie at the junction of tributaries with the main stream. In such cases it has sometimes happened that the water filling the secondary valleys has been pounded back by the greater flood coming down the main one, so that, after the gravels have been spread out by the latter, the contents of the former have been washed over them. And thus, pre-diluvial mammaliferous sands lying in the tributary valleys, and protected from the violence of the flood coming down the principal channel, have, by the change in the direction of the currents during the falling of the water-level, become interstratified with—or even shifted unto the top of—much newer deposits. The only way to guard against being deceived by instances of this kind is not to accept any evidence of superposition as conclusive where the bones of the same animals do not lie near together, where the lamellibranchiate shells have not their two valves united, or where there are other signs of the deposit being a drifted one.

In Norfolk and Suffolk the Middle Sands and Gravels often contain fragments of marine shells derived from the Weybourne Sands and other beds. In Yorkshire they are known as the Hessle Sands and Gravels.

B. *Land Surface.*—How long the gap in the ice-barrier remained open after the discharge of the water of the first lake it is impossible to say. We have evidence, however, that it was long enough for some of the land-shells to occupy the surface again, and also, as the buried forest-bed at Ealing witnesses, for small trees to grow up. Until very recently I had not obtained this evidence, and was under the impression that the lake had been quickly re-formed. The forest-bed at Ealing shows clearly, however, that some years at least must have elapsed before this took place. I have found land-shells at Brentford, and near Bletchley, in Buckinghamshire, at this horizon.

The Rev. H. M. De la Condamine described, in 1853, a deposit containing land and fresh-water shells on the top of the Middle Sands and Gravels, in the valley of the Ouse, between St. Ives and Huntingdon.* At Crayford there is a grey sandy clay containing shells of the fresh-water mollusks, *Planorbis*, *Bythinia*, *Lymnea*, and *Anodon*. The *Anodon* has the two valves united. The clay with these shells overlies the mammaliferous gravels and underlies the deposits that appear to be the representatives of the Upper Boulder-clay, so that I think it must belong to this stage. It was probably at this time that the land shells contained in the Upper Loess and Diluvium multiplied greatly, and occupied the land, many of their natural enemies and competitors having been destroyed by the flood.

It seems not impossible that remnants of palæolithic tribes or some of the great extinct mammals might have escaped destruction at the time of the rising of the waters of the first lake, and have spread over the area again before it was again submerged. I have, however, so far found no evidence whatever that they did so. The scarcity, if not absolute non-existence, of the bones of the species of deer, horse, and ox, that we know were not exterminated, makes it probable that the gap in the ice-barrier did not remain many years open.

A. *Upper Diluvium.*—The Upper Diluvium, under which name I include the Upper Boulder-clay, the Upper Loess, the Hessle Clay, and the Upper Brick-clays, is the most widespread and the best preserved of all the glacial deposits. The gap in the ice-barrier had been closed and the lake re-formed, but this time the water was not suddenly discharged, and gradually cut out a channel between the Black Sea and the Mediterranean. There are several facts pointing to the probability that it was the channel of the

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* Quart. Journ. Geol. Soc., vol. ix., p. 271.

Bosphorus that was thus formed, and not the Dardanelles. The deposits that were spread out during the continuance of the second lake were not subjected to the action of any sudden and tumultuous outpouring of the water, but only to its gradual subsidence. They have been but little denuded, and are still to be found nearly everywhere over the area that was submerged.

During the rise, greatest extension, and subsidence of the water, sediments, varying greatly in character, were formed. The earliest of all in the Ealing district—the first sign of the second rise of the flood—is the bed of silt enveloping the stumps of the small trees of the buried forest. The similar silty clays at Brentford and elsewhere, with land shells, mark the same event. There are, in the alternations of gravel and sandy clay in some of the sections at Ealing, signs of small oscillations of the level of the water, during which the subangular gravels were partly denuded, and furnished some of the materials for the new deposits. Above this we have a bed of clay, showing a greater depth of water, and stones scattered through it, indicating the agency of floating ice.

In addition to the single pebbles, and sometimes larger stones, there are patches of gravel and sand that appear to have been dropped in a frozen state into the bed of clay as it was forming. They are often angular, and show the lines of their original stratification, now lying at all angles or turned completely on end. They occur still more frequently in the Upper Boulder-clay of Norfolk and Suffolk than in the clay of the Thames Valley, and form one of the many evidences of the identity of the two deposits. The most probable explanation of their presence in the clay is, that the rising of the second lake took place wholly or partly in the winter season, and during an extreme frost; that ice was continually forming along the ever-widening shore, and, being broken up and floated off by the rising water, bearing masses of frozen gravel and sand with it; that thus a wide area of the lake was covered with gravel- and sand-laden ice, and that, on the melting of the latter, the frozen masses fell to the bottom and were imbedded in the clay.

Very little *detritus* that can be traced to a northern source has been found south of the Thames, and this has led some geologists to hold the opinion that the country south of the river was not submerged when the Upper Boulder-clay was spread out. It is difficult to believe that this can have been the case, when we find the drift covering the northern brow of the valley. Prof. Prestwich has also observed transported rocks on the top of Well Hill, in Kent, at 600 feet above the sea. Along with large rolled flints were found a few fragments of chert and ragstone, which he refers to the Lower Greensand of the Sevenoaks range, some six miles further south, separated by the broad and deep valley of Holmsdale.* Mr. Whitaker has also seen lumps of hand-chalk, in Kent, at a high level, on Crocken Hill, eastward of St. Mary's Cray;† and Prof. Morris has informed me that there are patches of the loess, with its characteristic shells, preserved in fissures of the chalk at Bensted's Quarry, near Maidstone.

We have an obvious explanation of the nearly total absence of northern drift south of the Thames in the theory that the Upper Diluvium was distributed by ice, floating over a great lake draining to the Mediterranean by way of the Black Sea; for the currents from the South of England would flow to the north of east, to get round the Hartz Mountains, and might be deflected still more to the north by those from the valley of the Rhine, and from the area of the English Channel.

III. ON THE POSSIBILITY OF EXPLAINING PAST CHANGES IN THE UNIVERSE BY CAUSES AT PRESENT IN OPERATION.

By S. Tolver Preston.

ANY attempt to explain the phenomena of Nature by the recognised working of physical causation has been invariably welcomed. Thus the explanation of geological changes through the influence of time, by the recognised working of natural causes, is now generally accepted with satisfaction, and the idea of cataclysms or catastrophes has been abandoned. Might not the same thing apply to cosmical changes (or to changes in the Universe), or would not an attempt to explain them by the recognised action of physical causation be as desirable as in the case of geological changes ?

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* Geological Magazine, October, 1874.
† Geology of London, p. 52.

One insuperable barrier in the way of explaining past changes in the Universe, by causes at present in operation, has been the supposition—in the absence of any explanation of the mechanism of gravity—that the range of gravity extends to indefinite distances, and consequently that every portion of the Universe tends to agglomerate or collect together into one mass, whereby all conditions of stability or permanence are excluded. Another difficulty has been to conceive how the energy (in the form of heat) dissipated by matter in the ether of space could ever again be made available or re-transferred to matter. Now, in the first place, it has never been *proved* that the range of gravity extends to indefinite distances, and, on examination, conclusions so incongruous are involved in this assumption that they themselves render the assumption unlikely. Thus imagine the incongruity of the idea of an *unstable* Universe tending to agglomerate into one (perhaps infinite) mass, gravity itself becoming (in the limit) infinite, so that the conditions of life would be rendered utterly impossible. Can it appear reasonable to conclude that physical causation should be so adapted as inevitably (in time) to produce such a state of things as this, or to involve instability and confusion ?

According to the only physical theory of gravitation which has yet withstood the test of criticism (viz., that of Le Sage*), and which has been received with some favour by some of the most eminent physicists, it is *certain* that the range of gravity cannot extend to indefinite distances. It is therefore quite possible that the range of gravity may not extend to the stellar distances so that the stars do not gravitate towards each other.† This would evidently be the condition required to satisfy the very important point of *stability* in the Universe. The range through which gravity has been actually observed to prevail is known to be infinitesimal compared with the stellar distances, so that the range of gravity may perfectly well lie within these limits. Thus the condition for *stability* is the first important conclusion following on the recognition of the *limited* range to gravity required by the physical theory.

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* In three papers published in the Philosophical Magazine (September and November, 1877, and February, 1878) it has been my object to show how Le Sage's original theory may be modified, and certain difficulties in the way of its acceptance (in its original form) removed by the light of the modern investigations connected with the kinetic theory of gases. A fourth paper (after the MS. of the present paper was written) has also been published in the Philosophical Magazine for April last.

† Of course we do not allude to *double* stars, in close range.

Since an explanation of the mechanism of gravity is attracting attention at the present time, Le Sage's theory (or a modified form of it) being apparently the only conceivable one that can satisfy the various conditions of the problem, it therefore behoves us to inquire—as a point of great interest, and for the furtherance of scientific truth—what modification the acceptance of this explanation of the mechanism of gravitation (and the important connected inference that its range is limited) would make in the views as to the operation of physical causation in the Universe; and this inquiry would appear to be all the more desirable in view of the accepted fact that gravitation is really the most important physical agency in the Universe, and therefore any modified views regarding its nature and range of action would naturally entail important deductions and modified views in regard to the working of natural phenomena.

The second fundamental conclusion that follows by the recognition that the range of gravity is *limited*, and is within the stellar distances, is that the stars must be moving in *straight lines*, and not in orbits, as supposed on the assumption of an indefinite range to gravity. It follows therefore, by the ordinary laws of probabilities, that if a star continue to move in a straight line for a sufficient time it must inevitably *come into collision* with another star situated somewhere in the line of the star's proper motion; or that, in general, collisions among the stars (in a state of proper motion among each other in straight lines) must be inevitable. This, therefore, may be regarded as the third fundamental conclusion that follows by the recognition of a limited range to gravity. In the "Quarterly Journal of Science" for July, 1877,* is a paper by Mr. James Croll, in which he calls attention to the fact that some explanation is required in order to bring the age of the sun's heat up to the length of time required by the teaching of the facts of geology, and has suggested the collision of matter (in a state of proper motion) for that purpose. The mere approach of the matter forming the sun under the action of gravity without a collision (due to a previously existing proper motion) appears to be demonstrably insufficient to account for the age of the sun's heat. It is therefore so far satisfactory to observe that the inference of the collision of masses (in a state of proper motion) arrived at on independent grounds by Mr. Croll is precisely the same conclusion that necessarily follows from the deduction that the range of gravity is *limited*, and therefore that the stars necessarily move among each other in *straight lines*, whereby, in the natural course of things, collisions are rendered inevitable. The eventuality of collisions among the stars is also treated of in a paper by Mr. Johnstone Stoney, "On the Physical Constitution of the Sun and Stars" (Proc. Roy. Soc., 1868-69).

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* Also Phil. Mag., 1868.

The question of difficulty that remains would be to explain how the colliding matter of the Universe is separated and prevented from aggregating together to an indefinite extent. The *limit* to the range of gravity and the *limit* to the intensity of gravity required by the physical theory would appear to have a bearing here. It may be observed that there are two opposing agencies, the expansive action of heat and the contractive action of gravity. By the continued aggregation of matter the expansive action of heat increases, while the intensity of the contractive action of gravity attains a final limit. It would therefore appear reasonable to conclude that a balance would naturally set in, at which the expansive action of heat would compensate the contractive action of gravity, and thus a natural limit to the aggregation of matter would be reached. Thus the general result led up to by these conclusions would appear to be that the parts of the Universe are in motion among themselves in straight lines (much in analogy to the molecules of a gas, as far as regards character of motion); that, under the collisions that occur, a general balance is maintained in the quantity of heat, and general distribution and state of aggregation of the matter; that some stellar suns are in the act of cooling down, while others are renovated by fresh collisions, changes continually occurring in parts of the Universe, but the whole remaining unchanged in character. That sudden developments of heat among the stars and (as it were) the flashing out of suns do occur at times, is a notorious fact in astronomy. It should be noted that the above fundamental deduction, that the parts of the Universe are moving in straight lines (which entails collisions), cannot be regarded as a mere speculation, but is a necessary inference following on the recognition that the range of gravity is *limited*, and on the fact that the stars are actually observed to possess proper motions in various directions. The deduction that the range of gravity is *limited* is itself a necessary consequence of the one explanation of the mechanism of gravity that has received support by competent judges.

From the deduction that the stars are moving in *straight lines* in various directions, the analogy (as regards character of motion) with a gas becomes natural and inevitable; for dynamical principles are independent of *size* or relative scale, or it is the same as if we were dealing with molecules observed to have a proper motion among each other in various directions. The analogy of a gas presents itself, therefore, rather as a necessary consequence, not as a speculation.* The *degree* of aggregation of the separate parts of the Universe moving among each other in straight lines would therefore appear to be necessarily dependent on the mean velocity of proper motion, just as the degree of aggregation of the components of the molecules of a compound gas depends on the mean velocity of their proper motion. If the velocity of the compound molecules of the gas be increased (which is synonymous with what is called "raising the temperature") they begin to separate into their components (or to become "dissociated"), and if the molecules have a very high complexity the extent to which this separation occurs (*i.e.*, the final *degree* of aggregation) depends on the temperature, or on the *velocity* of the molecules. If the velocity be very high (by a high "temperature.") the molecules split up—"dissociate"—into their *ultimate* components. By gradually lowering the temperature (velocity) the converse process may take place, or the components may gradually aggregate together to form massive molecules. This analogy is applicable also to the larger scale parts of the Universe moving in straight lines among each other, since dynamical principles are independent of scale. Such an excessive velocity of the component parts of the Universe (the stars) among each other is quite conceivable, at which the whole would break up into *single* molecules. By a lower velocity these molecules collect together under the action of gravity into states of aggregation, or groups, on the same principle (and perhaps even much on the same process) as the constituents of the molecules of a compound gas group together under "chemical" action. The range of "chemical" action, like the range of "gravific" action, is *limited;* the constituents of the stellar masses, which group together under the limited range of "gravity," being in this respect comparable to the constituents of the molecules of a compound gas which group together under the limited range of "chemical" action. Just as in the case of a compound gas (on account of some molecules possessing much higher velocities than others) the components of two molecules frequently become dissociated by a collision, and group together again in a different part of the gas; so in the Universe, where some stars (no doubt for analogous dynamical reasons) possess a higher proper motion than others, we have complete disintegration by collision in some parts, aggregation in others, continual change, but the *mean* aggregation remaining unchanged.

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* The *relative* velocity of the stellar masses (meaning by this the relative time taken by them to traverse a distance equal to their own diameters) is almost indefinitely slow compared with the case of the molecules of gases, which traverse a distance equal to many millions of times their own diameters in a single second. The absolute velocity of the stellar masses, however (as far as observed), is very great compared with that of the molecules of gases, and consequently the heat developed at their encounters would be very great. On account of the large multiple the distance of the stellar masses is of their diameters, they would, no doubt, traverse vast distances before an encounter, or their "mean length of path" would be very great; and (considering the relative length of time a stellar mass takes to traverse a distance equal to its own diameter) an enormous epoch of time would in general elapse between the encounters.

It may be observed that in principle, in order to explain the continued working of physical phenomena, or the continuance of change in the Universe, the existence of some process of *recurrence* is absolutely essential. The particular material that is utilised for the development of fresh suns or centres of heat must be the cooled down or dead material of former suns. For if this were not the fact there would be a continual accumulation of the matter of extinct or useless suns in the Universe, and processes of renewal and maintenance of the energy of the Universe would come to a deadlock in the absence of matter to operate upon. It therefore becomes absolutely necessary to show, in any attempt to explain the continued working of physical phenomena under present causes, how the material of extinct suns can—in accordance with recognised dynamical principles—be made available for the development of fresh suns (or centres of heat). This conclusion would seem to evolve itself naturally on the basis of the deductions following from the physical theory of gravitation—in regard to the motion of the stars taking place in straight lines, which involves collisions and an alternating renewal and loss of heat. This appears on broad principle the only conceivable way in which there should be *recurrence*, under the condition that the *same matter should be used again.* On account of the relatively enormous area of free space, compared with the relatively very minute portion of space occupied by each star, a stellar sun would, in the course of its proper motion in a straight line, probably as a rule traverse an immense distance before an encounter, and thus have time to cool down before the renovation of its heat by a collision; the dead or used-up material of former suns being thus available for fresh suns.

There is one point in connection with this subject that may be worth noticing:—The sun, as is known, is giving off an enormous amount of energy in the form of heat into the ether of space, no less than about 1720 foot-tons of energy being thrown off from every square foot of the sun per *second*. Can it be imagined that this enormous total of energy can be given off in a particular direction—*i.e.*, *from* the sun—without any reaction in the opposite direction; or would it be thought that if the sun were emitting all this energy from *one side only* there would be no reaction in the opposite direction? To us it seems incredible, whatever the constitution of the ether may be imagined to be, that there should be no reaction at all by this enormous total of energy thrown off.* Admitting that there is a slight reaction, then it is probable that it would not be absolutely equal all round the sun, owing to accidental irregularities in the temperature and radiating power of the materials of the sun's surface. If this reaction were only a few *grains* per square foot (due to 1720 foot-tons of energy thrown off per square foot, per second), the resultant or unbalanced reaction, owing to irregularities in the distribution of the sun's heat and radiating power, might amount to a considerable *total* quantity, owing to the enormous area of the sun. This reaction, even if relatively small, might, in acting for ages (millions of years), in the end sensibly affect the proper motion of the sun; or this might be a cause having some influence on the proper motion of the stellar suns, in addition to the repulsive action of the heat generated by the mutual collisions.

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* We do not mean to infer that the reaction due to radiation is the cause of the motion of the radiometer, as we think there are grounds for believing that (in the case of the relatively extremely feeble heating of the radiometer) it would not be a *sufficient* cause to produce the motion, or probably any measurable effect. There can be little doubt that the *effective* cause of the motion of the radiometer has been quite successfully traced to the action of the molecules of the residual gas, as shown by Mr. Johnstone Stoney. This, however, by no means proves that there is *no* reaction by radiation. In considering the reaction in the case of the sun, the relatively enormous value of the energy continually thrown off from its surface (amounting to thousands of horsepower per square *foot*) must be duly realised. It may be estimated that a portion of the sun's surface equal in size to the disk of an ordinary radiometer (say a quarter of a square inch in area) is throwing off a wave energy equal to twelve horse-power. A vertical column of ether of this sectional area, situated on the sun, is therefore transmitting the same energy as the belt of a twelve-horse steam-engine. Can it be imagined that it can do so without any reaction at all? Consider, also, the millions of square miles of the sun's surface, each minute portion of which is throwing off this same energy.

In the absence of any explanation of the mechanism of gravity, or idea of the nature of gravific energy, it naturally becomes impossible to see how heat once dissipated in the ether could ever be recovered again, or how heat-energy derived from gravific energy could ever be re-converted into gravific energy. But when it comes to be recognised (as it logically and inevitably must) that gravific energy resides in a medium pervading space, and that therefore this medium is necessarily immersed in the ether or heat-conveying medium, then we have data for new conclusions. It appears clear, or almost a necessary deduction, that the waves of heat cannot be conveyed through the gravific medium (in which the ether is immersed) without becoming to some extent dissipated in the gravific medium: for it seems incredible how two media should be immersed in each other without interfering with each other's motions at all. If we admit that the waves of heat, though no perceptible diminution occurs in planetary distances, are at last in the course of ages frittered away or dissipated in the gravific medium in which the ether is immersed, then, since energy cannot be annihilated, the full equivalent of heat-energy (represented by the waves) is finally converted into gravific energy (by dissipation in the gravific medium). The energy, therefore, which came from the gravific medium, and was converted into heat-energy in the action of gravity at the approach of the masses, is finally restored to the gravific medium again, in the frittering away of the waves of heat in that medium. Thus the total amounts of gravific energy and of heat-energy may remain constant in the Universe, the one being convertible into the other, and back again. It may be observed that, besides the above apparently unavoidable conversion of heat-energy into gravific energy by dissipation of the heat-waves in the gravific medium (and the consequent equalisation of the energy in the two media), it would seem a reasonable conclusion that two media (the gravific medium and the heat-conveying medium), *immersed* in each other, must naturally maintain an equilibrium of motion or energy between themselves, as, for example, is known to be the fact in the case of two gases immersed in each other or mixed, however diverse their qualities may be. It should be observed that the gravific medium *must* be constituted as a gas (according to the kinetic theory) in order to accord with the observed effects of gravity.

These considerations would point to the general conclusion that *recurring* changes take place in the Universe whereby the continuance of useful activity and life is insured, and the deadlock of useless inaction and uniform repose prevented. In other words, they would lead to the inference that physical causation is not so constituted as to defeat itself and bring the operations of the Universe to a standstill.

It should be noted that, as a matter of theoretic principle, the problem of recurring changes in the Universe undoubtedly admits of solution, since it is an admitted dynamical principle that masses immersed in media whose particles are in a state of translatory motion must themselves inevitably acquire *some* translatory motion, and the stellar masses moving freely in straight lines in the gravific medium represent (as it were) a larger scale gas immersed in a smaller scale gas (viz., the gravific medium). It is a mere question of *scale*, and dynamical principles are (admittedly) independent of scale. The difficulty, if any, would therefore be one of *degree*, not of principle. There might be a difficulty in accounting for the high value of the translatory motion of the stellar masses that observation appears to point to. But it should be noted that it does not follow that the value of the translatory motion we observe is the true *mean* value; for it might be exceptionally above it. For it is known that the translatory motion of the molecules of gases (or of any bodies moving freely among each other according to kinetic theory) varies from zero towards infinity. It might therefore well be that the translatory motion we observe in our immediate neighbourhood might be exceptionally above the mean value, and we happen to observe this *particular* value because it is suited to the conditions of life (or the low translatory motion, on account of the feeble heat developed, would not be so suited).*

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* Possibly there may be some supplementary cause tending to produce translatory motion in the stars. The spectroscope shows the molecules of matter to possess a considerable complexity, or their *parts* have a considerable capacity for taking up motion. It would appear reasonable to conclude that the molecules of matter of the Universe, immersed in a medium constituted according to the kinetic theory, must acquire also in their *parts* a certain degree of motion, owing to the dynamic action of the impinging particles of the medium in which they are immersed. The intensity of motion thus acquired would (as is known) be greater in proportion as the parts of the molecule which are capable of motion are smaller; and this motion of the parts of the molecules would apparently tend to produce translatory motion in the molecules as wholes—much as the development of motion in the *parts* of the molecules of a gas (as occurs when they are exposed to the pulsations of waves of heat) is known to produce translatory motion in the molecules as wholes, thereby producing expansion in the volume of gas. Indeed a balance

It may be remarked that the conditions above arrived at, as a basis for recurring changes in the Universe, were not deduced solely with this object in view; but these very conditions follow independently as necessary consequences of the explanation of gravitation afforded by the kinetic theory, so that it becomes a remarkable fact that the very conditions that follow on the explanation of gravitation are precisely of that character required to account for *recurring* changes in the Universe. The more the subject is reflected on, the more apparent will it become that *in broad principle* other conditions for producing *recurring* changes are not conceivable—keeping in view the necessity (in order to effect recurrence) that the cooled down material of the Universe should be that material which is utilised for the development of fresh centres of heat; and there would appear to be a simple grandeur (not out of harmony with the recognised characteristics of Nature) in this great result being brought about by the mere movement of the stars according to the kinetic theory. Also there would be a sort of harmony or

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(as is known) tends to establish itself between the energy of the translatory motion of the molecules as wholes, and the energy of the motion of their parts (internal motions). Thus the throwing of the parts of molecules into motion (from any cause) would tend to produce motion in the molecules as wholes. In principle, therefore, the development of motion in the smaller parts of matter tends to produce translatory motion in the larger parts (and that whatever the relative scale may be). This might possibly have its application as an auxiliary cause for the development of translatory motion in the larger scale parts of matter (represented by the stellar masses), through the intermediary of the smaller parts (molecules), and the minute moving particles of the medium in which these smaller parts are immersed.

Another point may perhaps be worth noticing here. When the molecules of a compound gas (immersed in another gas) break up from any cause into their components, each of these smaller components tends to acquire from the rest of the gas the same *absolute* kinetic energy of translatory motion as the entire compound molecule possessed (before it was broken up); so that therefore the total energy of a given portion of gas may be suddenly greatly augmented by the mere breaking up of its (compound) molecules into parts. If we imagine any limited portion of a gas to be immersed in another gas of unlimited extent (or if the portion of gas be supposed enclosed in some way so as to permit expansion, and at the same time to allow a free exchange of energy between it and the other gas), then a change in the state of aggregation—by the breaking up into parts of the compound molecules of the enclosed gas—will be followed by a considerable transference of energy from the outer gas to the enclosed gas; for when equilibrium is again attained, each of the small parts into which the compound molecules of the enclosed gas are split up will have acquired from the outer gas the same absolute kinetic energy of translatory motion as the *entire* molecule possessed before it was broken up. Thus the mere act of breaking up (disintegration) of matter, *immersed in a medium enclosing a store of motion*, may cause considerable transference of energy from this medium to the immersed matter, tending to produce expansion and rebound. Possibly this principle might also have some bearing on the disintegrating changes continually taking place in the Universe.

analogy in the stellar masses and the media in which they are immersed moving under the same dynamical principles. Moreover, the kinetic theory has been mathematically proved (when a large number of masses are concerned) to produce a system of order and symmetry (or *mean* similarity of the conditions in all parts of the system) which is rigidly and automatically maintained by a process of self-correction under dynamical principles—a self-acting adjustment of the motions continually taking place, whereby a system of harmony and order is maintained everywhere, a perfect state of mobile equilibrium existing in all parts.

Those who regard the physical causation of the past in the light of the physical causation of the present, or who look upon the principle of the conservation of energy as a truth as necessary in the past as in the present (or who look upon physical truths as independent of time), are bound to believe that some process of recurrence must exist, whereby useful change and activity are continued in the Universe, and the purposeless end of a changeless chaos prevented; and that we should seek for the explanation of this, not so much with the view to prove the fact thereby, but rather as a satisfaction or confirmation of a fact we already had logical grounds for believing to exist. As we happen to have given considerable time and thought to the problem of gravitation on the basis of Le Sage's fundamental idea, we have considered ourselves justified in calling attention to the important consequences and modified views regarding natural phenomena that the acceptance of the physical theory of gravitation entails.

The phenomena of Nature would thus consist in *cyclical* processes involving transferences of motion from physical media in space to matter, and back again to these media (in a circle), the collective sum total of energy in matter and in these media remaining at every instant constant, in accordance with the principle of the conservation of energy. The cyclical processes thus taking place on a great scale in the Universe are imitated on a smaller scale in the ordinary interchanges of energy on the earth's surface, or all the ordinary processes of "work," or the derivation of power, show themselves to be *cyclical* processes on the recognition of the stores of motion in space rendered necessary by the physical theory of gravitation. For suppose, for example, we derive power from the fall of water, and use it through machinery for lifting weights or elevating materials,—then the motion derived at the descent of the water comes from the store of motion in the gravific medium, and passes to the gravific medium again in a *cyclical* process, in the operation of elevating the materials in opposition to the dynamic action of the gravific medium which tends to urge them towards the earth. If, instead of elevating materials, the work be done in friction, as in dragging masses (vehicles, &c.) along the earth's surface, then the waves of heat due to the friction are radiated into the media of space, and, becoming naturally dissipated in the gravific medium through which they have to pass, the energy thus finally returns to this medium, to be available for utilisation again. Winds or currents of air are due directly (as is known) to variations of pressure in the earth's atmosphere, which re-adjust themselves through the action of gravity. The gravific medium is therefore the agent directly concerned in producing the current of air or wind, and therefore is the motive agent concerned in developing motion in the machinery driven through the intervention of the wind. In the case of the ship propelled by the wind, the power is converted into heat in the passage of the ship through the water. The heat then passes in waves to the media in space, or is dissipated in the source whence it was derived in a *cyclical* process. It should be observed that all these deductions are necessarily true on the acceptance of the explanation of the mechanism of gravity afforded by the physical theory.

There can be little doubt that the explanation of gravity (the fundamental agency in Nature) will naturally and inevitably entail with it, in principle, analogous explanations of the other molecular motions ("chemical" reactions, &c.). Thus the operation of the steam-engine will show itself as a *cyclical* process, the motion passing from the media in space to the coal, and thence (in a circle) to the media in space in the various operations of the engine.* Thus considerations of great practical interest present themselves on the recognition of the existence of the stores of concealed motion in space revealed by the kinetic theory of gravitation, and problems of the highest practical importance may suggest themselves as to possible methods of utilising this store of motion to greater advantage than at present. There might be a tendency to regard considerations of this nature (independently of their truth or error) as in some degree *occult*. This could only arise from the kind of prejudice that tends to beset every new path, or possibly in the absence of realisation of the *mechanical fitness* of the scheme due to *attention* not having been given to the subject. What may be justly regarded as occult are the effects themselves (or the developments of motion in matter, occurring on every hand) without any explanation at all. Since the store of energy simply consists of finely subdivided matter in a state of rapid motion, and since it is obviously just as easy to reason of matter of one dimension as of another, there can be nothing whatever occult about the subject at all. The conclusion deducible with certainty beforehand that this store of motion—*if* it did exist in space—*would be concealed*, ought to go far to remove all preliminary doubts as to its existence; and unless we are ready to surrender the right of using our reason, the developments of motion occurring on all sides (as combustion, the detonation of explosives, and the varied movements developed in masses and molecules of matter, &c.) are utterly inexplicable without the existence of this store of motion in space. Minuteness of size of the particles is evidently adapted to rapidity of motion, and this rapidity of motion is itself essential to an intense store of energy, and this rapidity of motion, combined with minuteness of size, obviously and necessarily renders the motion of the particles concealed; indeed the more perfect the concealment, the higher should we be warranted in estimating the intensity of the store of energy to be (from the known fact that the higher the velocity of the component particles, the more impalpable does the medium become). It is very important, therefore, to keep in view that—independently of all question of the existence of this store of energy in space—it is so far an indisputable fact that *if* it did exist it would be perfectly concealed. There can therefore be no logical *à priori* reason for doubting its existence, and the admirable manner in which (on analysis) the mechanical conditions show themselves to be adapted to allow the existence of an available store of energy in space, to any intensity consistent with concealment, and in harmony with the conditions of life, ought to render the contemplation of the problem one of great mechanical interest. Surely, for example, to realise how the motion is transferred from the concealed store of motion in space to a mass of gunpowder (as a shell for instance) in the act of explosion, is a problem of the highest mechanical interest, and no one surely would mistake theories which may serve as a convenient temporary refuge in the absence of any conception of the process involved, as *explanations* of the process involved. Clearness of conception is the test of truth, and constitutes the real dignity of Science, and theories, however elaborated, if vague, have no real dignity; and the aim ought surely to be to endeavour to think out the *simplest* method of arriving at a result, keeping in view the fact that simplicity is *necessary* to the orderly working of mechanical processes, and that the real point to admire is a *simple* method of attaining a mechanical result, because it is *unique*, not a complicated method (which may be conceived to be indefinitely varied). The beautiful kinetic theory of gases affords the simplest method to the solution of the problem of the constitution of the physical media in space, and in the case of the gravific medium it may be proved to be the only solution which can harmonise with observed facts.*

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* See also a paper in Philosophical Magazine for April last ("The Bearing of the Kinetic Theory of Gravitation on the Phenomena of 'Cohesion' and 'Chemical Action'").

When we realise the practically unlimited intensity of the concealed energy that may thus exist in space (in the simple form of finely subdivided matter possessing a high velocity) one may cease to wonder at the sudden energy transferred to a mass of gunpowder (or a shell) in the act of explosion, and realise how this otherwise incomprehensible and extraordinary effect can take place. It would be illogical to consider a fact less extraordinary, or that a rational explanation for it is less urgent, because it is commonplace, and precisely because there is this tendency it ought to be specially guarded against. In the case of the explosion we have in principle simply the transference of motion from minute particles of matter (where the motion is therefore invisible) to grosser matter, in the form of clouds of vapour and palpable fragments of matter (when the motion is visible); and therefore to the superficial bodily eye there naturally *appears* to be an actual *creation* of motion. If the motion (before transference) were not invisible, it would not be *mechanically efficient* for the object to be attained; for unless the moving particles of matter constituting the medium were small, it would be impossible to *concentrate* a considerable intensity of energy within a small volume of space. In the case of any powerful or intense motive source, *velocity* of the particles must evidently be relied on rather than mass (since mass occupies space, and prevents concentration of the energy. There could be no logical ground why problems of this nature should be regarded in

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* It may be observed that in the case of *any* medium constituted according to the kinetic theory, the velocity of the particles of the medium is equal to the velocity of propagation of a wave in the medium—

$$\times \frac{3}{\sqrt{5}}.$$

See result appended by Prof. Maxwell to paper "On the Mode of Propagation of Sound" (Philosophical Magazine, June, 1877).

any other light than as ordinary engineering or mechanical problems, to be explained by the light of reason and common sense. There would appear to be a kind of prejudice in regard to thinking on these subjects which requires to be surmounted or dissipated by logic. It may seem bold to say it, but scientific *reform* is called for in this respect. Ability is by itself of no avail without *attention*. Reform, in the sense of a more general attention being directed to subjects whose real interest would disclose itself on examination, is what is required.

In regard to the subject of recurring changes in the Universe, this opinion appears to have been held by Sir W. Grove (" Correlation of Physical Forces," p. 67), though he does not go into any explanation as to the particular conditions required to bring about the result. He remarks relative to this subject (p. 67) :—" Enlarged observation may prove that phenomena seeming to tend in one direction will turn out to be recurrent, though never absolutely identical in their recurrence; that there is throughout the Universe gradual change, but no finality; . . . that no star or planet could at any time be said to be created or destroyed, or to be in a state of absolute stability, but that some may be increasing, others dwindling away, and so throughout the Universe, in the past as in the future."

Humboldt also says, as regards this point (Preface to " Cosmos ")—" I would therefore venture to hope that an attempt to delineate Nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating ever-recurring alternation of physical metamorphoses, will not be wholly disregarded at a future age."

IV. THE EVOLUTION OF BEAUTY.

By F. T. Mott, F.R.G.S.

TO estimate the comparative value of Constitution and Education in producing the net result of character is one of the most difficult problems of physiology.

The influence of Education being the more easily recognised of the two, it is probable that this factor has rarely been undervalued, and the Darwinian philosophy has turned upon it a special share of attention. Inherited "tendencies" are indeed admitted by modern evolutionists, but with a very vague conception of what it is which "tends;" and generally the evolving organism is treated rather as a *tabula rasa*, on which surrounding conditions paint themselves in a cumulative manner, than as a centre of force to whose inherent activity is largely due the character of the complex product.

The existence of a "vital force," under some name or other, is not perhaps denied, but it is comparatively ignored; the main arguments being expended in attempting to show that the variations of form, colour, ornament, and disposition may be accounted for by the mere action of surrounding conditions; that Education is everything, original Constitution of quite secondary importance. Yet the fact is surely evident that every organism has within itself a force which will expend its activity in a definite direction, and that its environment has only power to modify that line of activity within very narrow limits.

An acorn cannot be made to grow into anything but an oak, with perhaps some small variation of form or colour; and though it be granted that variations in the same direction, impressed upon many successive generations, may effect large changes in the end, this is the result of guiding rather than of creating. It is the turning of the stream into a new channel, not the making of the river; and the most important factor in the product is, after all, the current with its perpetual activity, rather than the physical conditions to which it partially submits.

A recent article in the "Quarterly Journal of Science," on "The Action of Light upon the Colouration of the Organic World," presents a fair example of the manner in which this question is commonly treated. Impartiality and sound logic are not wanting, but the argument is invalidated because it ignores one of the primary factors of the problem.

To discuss the manner in which the surface-colouring of plants and animals is produced by insects or by light, without reference to the internal forces upon which these influences act, is like calculating the velocity of the hands of a watch upon the basis of the decreased temperature by which it is slightly accelerated, without taking account of the spring, or of the fact that the velocity will remain nearly the same whether the temperature be lowered or elevated.

Is it not possible to get a more satisfactory view of organic phenomena by going a step lower in the chain of causation? By turning our attention to the internal sources of activity, as well as to external influences, and by endeavouring to estimate each factor at its true value?

We have no knowledge of Matter, except as accompanied by Force. The ultimate atoms we imagine to be related to each other by attractions and repulsions. The compound molecule is the seat of equally compound Forces. The possibility of such a perfect balance of Forces as to produce absolute rest may be argued, but can scarcely be proved, and a probability remains that no such thing as absolute rest is known throughout the Universe. Everywhere the concomitant of Force is motion or activity, either atomic, molecular, or molar; and the universal character of such activity is *alternate acceleration and retardation.*

We need not discuss the problem whether the Force or the Matter is the actual *substans.* The same general laws of activity hold good both for Matter and Mind, and one of the most fundamental of these laws is that of alternate acceleration and retardation, of expansion and compression, of increase and decrease, whether in relation to time, to space, or to intensity. In modern terminology this type of activity is known as *wave-motion,* and the total activity of the Universe, as far as we can penetrate it, is made up of such waves, in what may be called—without extravagant metaphor—an "infinite" variety.

The wave form originates in the compound action of initial impulse with surrounding resistances. Both factors are essential, and if space is everywhere occupied by matter, or is everywhere under the influence of attractions and repulsions, there can be no initial impulse which is not instantly met by resistance, no motion therefore which does not partake of the wave character.

An initial impulse, if unresisted, would, it is supposed, continue with unvarying velocity and direction through space and time for ever. But by the action of surrounding resistances, which are themselves perhaps but modifications of other initial impulses, its equilibrium is upset, its velocities made inconstant, its direction and intensity altered, divided, scattered, or concentrated, in a thousand different ways. How the initial impulses originated we need not inquire. We have to deal with a Universe in which they have been operating practically for infinite time, and to estimate as accurately as we can their present condition.

It seems correct to speak of these impulses as separate and individual phenomena, because, although the whole

Force of the Universe is probably a unity, it presents itself to our senses and understandings as divided everywhere into distinct units. In their simplest form we conceive of these units of Force as associated with material "atoms."

Wherever heat exists the material atoms are supposed to be in a state of perpetual agitation, and every minute atomic movement originates a "wave" of force in its simplest character, the differences between such an atomic wave and the great secular waves with which astronomy and geology mainly deal being probably differences due to increased intensity and complexity alone.

The *normal life* of a wave dates from the starting-point of its initial impulse to its maximum of compression or retardation, and thence through the period of reaction till it returns to its original condition. But in vast numbers of cases this normal life is not completely fulfilled. The surrounding conditions being continually varied, in a succession of similar impulses no two will meet precisely the same fate. Many of them will be checked at different periods of their career, absorbed, divided, repelled, turned aside, or compounded with others; and out of this complex action new impulses will arise to pass through similar changes, and so keep up the perpetual agitation and life of the material Universe.

The *forms* of wave-motion may be indefinitely varied, but the three leading types are—(*a*) the wave of oscillation; (*b*) the wave of undulation; (*c*) the centripetal wave; the wave of oscillation being represented by the simplest atomic movement, while all compound movements tend towards the undulatory or the centripetal forms.

A simple atomic vibration in a molecule of unstable composition may be followed by molecular change and recomposition, the effects of which are passed on through surrounding matter in a succession of compound waves. These compound waves, meeting everywhere with other compound waves of varied form and intensity, may either be dissipated early in their career, or may pass through their normal life, or may be taken up without dissipation into the life of another compound wave of greater intensity or wider scope. By this latter process of absorption, combined with the reverse process of subdivision, a wave of great initial intensity may become complex to a degree scarcely conceivable by the human understanding.

From atomic vibration to a solar system is a long leap, but it is no more than the result of such composition of wave-motion carried on through indefinite time.

From a solar system to an acorn is a long leap also, yet it is probable that this, too, is only a result of the same law, only a still further development of the complexity of wave-motion. An organic unit seems to be the latest outcome of compounded force—the most complex form of wave yet attained to, in at least this corner of the Universe.

If we examine the structure of a complex wave we find that it consists of an organised body of waves, arranged after the manner of a disciplined army. There is first a single inclusive impulse, of the highest intensity and widest scope, holding in its grasp the total body of subsidiary waves, and representing the General in command. Then there are a few large, well-marked, secondary impulses, like separate brigades, working towards special goals, but with a common end in view. Each of these secondary waves is made up of a number of tertiary waves, which may stand for regiments, and within each regiment are again companies, squads, and finally individuals, the number of component waves increasing with each step downwards. But this organised arrangement of a crowd of units is not the only kind of complexity to be considered. Besides having its definite place in the system, each subsidiary wave has also its own normal period of life, and its own *velocity*, which varies not only in its totality, but in its *two phases*, viz., the ascending phase, in which it approaches towards its climax, and the descending phase, in which it recedes from it. These phases may either be equal or only equivalent, one being long and slow, while the other is short and rapid, the varieties of proportion in the latter case being unlimited.

Note what takes place in the growth of an acorn. While it lies brown and dry upon the earth, apparently lifeless and motionless, atomic and molecular motions are unceasingly at work among its tissues. This is proved by the fact that if left dry too long the living forces will be dissipated, the germ will perish and decay. But the presence of moisture and the communicated energy of external heat give rise to a re-composition of forces without dissipation. A wave of high intensity and rapidly increasing complexity is originated. Surrounding waves of force are absorbed into its vortex, and material molecules are built up in the track of the advancing wave, and of each unit in all its army of subsidiary waves. The normal form of the great inclusive impulse is such as to build up that material structure which we call an oak tree, and nothing else. The wave originated from a germinating acorn is always of that form, and no other. Its subsidiary waves, although unable to escape from the general law impressed upon them by the primary impulse, assert their individuality within that limit, and are slightly modified at the same time, as is also the primary itself, by surrounding resistances and attractions. Hence the result that no two oak trees are precisely similar in details, though identical in general plan.

Of the three leading wave-types, by far the most complex is the *centripetal*. This type is especially associated with organic life. The primary impulse in every entire organism is of this form; so also are all the great subsidiary waves. An organic being is a living and active centre, around which force and matter are alternately concentrated and dispersed. The normal life-period of each organism depends upon the character of its primary centripetal force-wave; and the apex or grand climacteric of its life is the point of greatest concentration, from which point dispersion sets in, ending in partial or in total dissolution.

The subsidiary waves being also of the centripetal type, but of varied periods and intensities, their climacterics are not synchronous, but successive, so that nearly always some portions of the organism are at their climax, while some are tending towards decay, and others still developing.

Among the characteristics of the organic centripetal wave there is one of peculiar significance. It is this—that the necessary result of centripetal action is concentration, and concentration implies the bringing into closer relations with each other of elements which had previously been more scattered. There is an important correlation between this process and our human senses.

Organic development takes place by the process of assimilation, by continually adding to the original impulse the energy of surrounding materials, by complicating the primary wave with an ever-increasing army of subsidiary waves. The intense attractive power of the organic life-wave has this especial capacity of accumulation and concentration, which is continued until the whole kinetic energy of the primary wave is changed into the potential form. At this point reaction sets in, and the descending phase of the wave commences, accompanied by decay of the organism and re-dispersion of its constituents.

During the career of the primary wave many subsidiary waves of all ranks will have run their course. In the life of a deciduous tree the *annual* wave, accompanied by evolution of foliage and blossom, and leaving part of its energy stored up in the seed, has almost a primary character, though it is in fact a subsidiary of the larger wave which carries the tree through many seasons to its normal limit of growth. But it furnishes also an analogy suggesting that even the primary life-wave of the individual organism is in truth subsidiary to a still larger and more inclusive wave controlling the development of the *species;* that each specific wave may be part only of a *generic* wave, which in its turn may be included in some wave of longer period and wider sweep; and that so all finite waves may, in their ultimate relationships, be but ripples of the one universal impulse.

Organic growth is, then, always a process of concentration, an accumulation of minute waves of force, each accompanied by some particle of matter. Of these minute waves many probably run together, and form the subsidiary waves of various ranks, while others remain distinct as superficial undulations. The constant tendency is to bring into relationship a vast number of force-waves which were previously scattered. These constituents are arranged, as they arrive, first in a rough outline indicating the tendencies of the primary wave; and this outline is gradually filled up as force and matter are accumulated, and drawn closer to the point of ultimate concentration of the primary wave.

In all developing organisms the order of growth is from the general to the particular, from the less to the more differentiated, from the scattered to the concentrated, from the outline or framework to the clothed and completed form; and although organic growth may appear to be an expansion rather than a concentration, it is not so in reality. When the bugle-call of a regiment in the field summons the skirmishers to retire upon their supports, the result is a concentration of force, although accompanied by an enlargement of the central mass.

In the process of organic development there appear to be at least four well-marked stages, which seem to indicate the existence of four large secondary waves immediately subsidiary to each organic primary. In the animal kingdom these mark the development of the four great systems of organic tissue—the cellular, the osseous, the fibrous, and the nervous. In the vegetable kingdom they are represented by the cellular tissue, the trunk and branch system, the foliage, and the blossom.

The *visible beauty* of the organic world depends upon the correlation between the sense-organs of the human race and these concentrating organic waves of force. That an object should appear to be beautiful is not the result of accidental surroundings, nor of any superficial garment spread over an ugly and repulsive interior. The elements of the beautiful are inherent in all things; that we cannot always recognise them is due to the limitation of our senses.

Beauty is an abstract idea, of the same nature as Goodness, Truth, Power, Charity, &c., and that which causes this idea to present itself in the human consciousness is *the perception of relationship among a number of diverse sensations*, of unity coexistent with variety. The mental sense of *ordered activity* is always accompanied by the idea of Beauty more or less vividly impressed. When the attention of the mind is focussed upon a variety of points in rapid succession, and the intellect is able to recognise *relationship* among all those points as members of one group, then arises the idea of Beauty. It can only present itself under the conditions of mental activity coexistent with the perception of relationship, proportion, unity.

Variety is necessary in order to secure the condition of mental activity, which is effected by the perpetual change implied in the successive contemplation of many points. Each act of attention is a force-wave which is very rapidly dissipated. No natural phenomenon can ever be exactly repeated; but if there is repetition, with too little variation, temporary paralysis follows, the well-known result of monotonous sensation. To keep up a vigorous mental activity the force-waves of attention must be sufficiently varied. Hence it follows that the primary condition under which any object can appear beautiful to the human mind is that it be compounded of a variety of parts, and that these parts be so much varied that the mind which contemplates them is not paralysed by monotony. Every object in Nature is so compounded of various parts, but human minds are not equally sensitive to small shades of difference. Obtuse minds are paralysed by a succession of acts of attention whose difference is sufficient to keep wide awake and active other minds of more delicate perceptivity. If any mind were absolutely sensitive to all shades of difference the paralysis due to monotony would be impossible to it.

Activity alone, however, is not sufficient to arouse the idea of Beauty; it must be recognised as *ordered* activity. The mind requires not only the perception of change, but also of relationship in passing from one act of attention to another, the perception that there is *similarity* as well as difference in those acts of attention. Similarity implies *identity* in at least one direction, with difference in other directions; and the point of identity which must be perceived in order that the group may appear beautiful is, that all those phenomena *belong* to that group, all take a necessary part in its formation, are bound together by a common bond, and form one unity. There may be many aspects of unity in a single group of concrete phenomena,—as unity of form, unity of colour, unity of motion, unity of purpose, &c.,—and the vividness with which the idea of Beauty is presented will be intensified as both variety and unity are perceived in a larger number of such aspects.

As different minds vary in their sensitiveness to differences between the acts of attention, so they vary also in their sensitiveness to *relationship* among those acts. A group of phenomena may appear to one mind to be closely related, while to another no element of identity among the varied parts can be perceived, so that the group appears a chaos, and the idea of Beauty is not evolved. Every object in Nature is a group of parts related to each other in ways more or less complex and subtle. If any mind were absolutely sensitive to all degrees of relationship, in all its aspects, nothing would appear chaotic. A mind absolutely sensitive to all shades of difference, and to all degrees of relationship at the same time, would see everywhere throughout creation variety bound up in unity, would find neither monotony nor chaos, discord nor ugliness, but only a universal Beauty.

The human mind, however, as it actually exists, has no such absolute sensitiveness. It is, in fact, sensitive either to variety or to unity only within narrow limits. Whatever groups of phenomena present variations or similarities within the range of those limits may be recognised as beautiful, but no others. To the naked eye no differences can be recognised among the minute grains which constitute the seed of the common musk-plant; and to the ordinary mind there appears at first to be no relationship between four such numbers as 264, 330, 396, 462; while the simplest of us can see that the stones on a gravel-walk are not all alike, and that the numbers 2, 4, 6, 8 are related to each other by a common law.

There is no doubt that culture increases the sensitiveness of the mind both to difference and to similarity, and it is certain that the cultured mind perceives more beauty in the Universe than the savage or the untaught. But the universal Beauty which exists throughout every corner of creation may be brought within the sphere of man's recognition by *changes in the grouping* of material forms, as well as by education of the mind.

Reduce to their simplest form the four numbers 264, 330, 396, 462, by dividing them by their greatest common measure, viz., 66, and you have the series 4, 5, 6, 7, the relations of which are perceptible at once to the most ordinary mind. Place before a person ignorant of plants the little yellow celandine and a red pæony, and he would not recognise any family resemblance; but fill up the gap between them with a buttercup, a globe-flower, and a Christmas rose, and he would perceive the gradation from one to the other and admit the relationship.

The closer the relationship, the more easily is it recognised; therefore the objects which appear beautiful to human minds must be those in which the parts are closely related, the proportions simple, the design obvious. Among mathematical figures the pentagon is one of the most elegant, because there is in its outline a sufficient variety with such simple proportions that the unity is easily grasped. The circle also is beautiful, so is the hexagon, the square, and the equilateral triangle; but in each of these the variety is less than in the pentagon. The dodecahedron, of twelve pentagons, is scarcely beautiful, except to a mathematician, because the variety is so confusing that part of the unity is lost, and the figure appears almost chaotic.

Divide a hexagon into six equilateral triangles, and let these be acted upon by an accelerated repulsive force so that they are driven radially from the centre to distances represented by the numbers 1, 3, 6, 10, 15, 21. They will then stand in a spiral of such form that the majority of persons would not recognise any law of relationship binding them into one group. Let, now, the repulsive force be changed into an attractive one whose formula is precisely reversed, and, as the triangles are drawn gradually towards the centre, their relationship will become more and more easily discerned, till, as they close up into the original hexagon, every human mind will perceive the unity of the group, and a sense of beauty will inevitably follow.

It is the character of the centripetal organic wave to be continually simplifying the relationship of its component waves with each other, both by concentration and the filling up of gaps; and it results from this that the nearer any organic wave approaches towards its climax, the closer are the relations of the component waves, and the more readily will its beauty be perceived by the human mind.

Beauty is thus shown to be an index of organic maturity, and not to depend upon any accidental or external influence, but to be inherent in every object, and only unseen during its embryonic stages.

The blossoming of plants indicates that the climacteric of their highest secondary life-wave has been attained. From that point reaction and sleep or decay set in. In perennials the great primary life-wave keeps its course through many flowering seasons; but there comes one season in which its power to blossom is at its height: this is the ultimate climacteric, and from that point there is gradual decay of the whole organism.

The *specific wave*, however, does not terminate with the individual. It is carried through the seed from generation to generation, and its climacteric is not reached until the highest blossoming power of the species has been attained. When this happens the species itself must gradually pass away.

The four great secondary organic waves, the cellular, the osseous, the fibrous, and the nervous, may exist in *unequal proportions* in a specific wave. The cellular is always first developed, afterwards the others in succession. But the grand climacteric of organic development is not attained until all four have reached the highest stage of concentration possible to them; and it is probable that such conditions have never yet existed on this earth.

Geologic history shows us, in vegetable life, first, a wonderful development of the cellular wave in the cryptogamic type, everywhere predominant; and then a similar epoch in which the osseous wave overtopped the cellular, clothing the world with mighty Conifers—huge masses of trunk and branches, with little foliage or blossom.

This was succeeded by an epoch of fibrous development, represented in the vegetable world by foliage. Forests of the broad-leaved Amentiferæ, the oak, the birch, the poplar, and the alder,—with elm, and maple, and plane, and other trees conspicuous for foliage, but not for blossom,—became the striking feature of the landscape.

Finally, as the nervous wave advanced towards its climax, blossom began to be developed in varied and conspicuous forms. Magnolias, roses, mallows, lilies, orchids, and many other "flowering" plants, appear in the latest tertiary deposits; and plants of this type still adorn the world, and probably become more numerous and more beautiful century by century.

It has been shown that plants with inconspicuous blossom have the widest geographic range, that those with white flowers have a range more limited, while species with brilliantly coloured blossoms have a still smaller range of distribution. Taking the area of distribution to indicate roughly the length of time during which a species has been in existence, these facts go to prove that the nervous wave of vegetable life, represented by the blossom,—the most sensitive, the most delicate, the most complex, and the most vitalised of vegetable organs, is still approaching its climax, and has not yet attained to it. They show, also, that visible beauty is to us the index of approaching climax, and not a quality which can be added at any epoch at which a temporary utility might seem to demand it.

In the light of this reasoning, the doctrine that "except for insects we should have had no flowers" cannot be maintained. Insects have doubtless aided the development of the life-wave among the complex resistances through which it has to make its way, but the beauty of the world, of which flowers form so interesting a part, depends upon laws far more profound, more deep-rooted and far-reaching, and which would surely have attained in due time their ultimate goal, even though the race of insects had never formed part of the same marvellous and admirable Cosmos.

V. FEELING AND ENERGY: ALTERNATE AFFECTIONS OF MATTER.

By W. S. Duncan.

IN a former paper I boldly controverted the hypothesis of concomitance between the mental and physical states attendant on nervous action. That hypothesis I endeavoured to show was inimical to a perfect psychological philosophy on a scientific basis, inasmuch as it placed an impassable gulf between consciousness and action, between feeling and energy. It was opposed to universal experience and language. It even jarred with the best system of ethics, based on the *happiness* of mankind.

With equal boldness I ventured to advance a rival hypothesis, namely, that Feeling and Energy could be regarded as strictly alternate and convertible affections of matter—a hypothesis which not only agreed with the language of mankind, scientific and untaught alike, but bridged the logical gulf, and accorded with the laws of continuity and causation in all their manifestations.

Anticipating some objections that might be urged against the view I had taken, I briefly stated and endeavoured to refute them. The importance of the subject, however, demands a closer examination of its bearings; and the object of this paper is to deal with some of the more salient objections liable to be urged by a careful critic.

At the outset I would desire to be distinctly understood as making no claim to *prove* the existence of feeling in any object, organised or not organised. We *infer* feeling to exist in our fellow-creatures, by observing actions performed by them which in ourselves seem to be the outcome of feeling. The inference may be a correct one, but it rests on analogical reasoning which is liable to error. Neither the denial of feeling, on the one hand, nor its affirmation, on the other, are valid in establishing a fact which from its very nature is incapable of absolute proof.

But while admitting this much, I am not precluded from submitting the question to a philosophical examination in view of discovering truth, which, though not self-evident nor perfectly demonstrable, may yet be supported by fair deduction.

Starting, then, with the assumption that MATTER in an *organised* form and possessed of life has the quality, or affection, or property of Feeling, I think it is only the first step towards consistency to admit that feeling being found in that form of matter must have previously existed as a fundamental property in matter before it became organised, and must remain in matter after disorganisation has occurred. For it is utterly out of keeping with all scientific teaching to believe that an entirely new thing like feeling, a state believed by many to be unique, or without parallel in any other aspect of Nature, should be the mere product of organisation. What is organisation? Unless we change the meaning of the term it implies but the combination, in an orderly connection, of elements which—with all their individual properties—existed before organisation. It is therefore as illogical to regard matter not organised as devoid of feeling, as it is illogical to attempt theoretically to evolve it, after a creational fashion, as a "something out of nothing," —a something *created* by organisation,—called into being out of nothing, by a mere process of orderly arrangement! Feeling, being found in organised matter, must exist fundamentally as an affection of matter in all its forms.

A superficial objection to this conclusion, not likely to be seriously urged by thorough-going philosophers of the type of Herbert Spencer, is the absence of "purposive" action in all inorganic forms of matter. The absence of purposive action in the operations of chemical combination, the propagation of heat, light, and electricity, the play of gravitation, and the distribution of forces by machinery, seem all to the common mind utterly inconsistent with the presence of feeling in the unorganised forms of matter in which these physical manifestations are displayed. The philosopher would reply to this objection that absence of purposive action did not necessarily imply the absence of feeling, any more than that the actions of a madman destitute of purpose proved the madman to be without feeling. It is the characteristic of the speech and action accompanying insanity in its most acute forms to be purposeless. Indeed the actions of a lunatic seem to be far more erratic and unexpected than the ordinary physical activities going on in the inorganic world. Yet the absence of purpose in the lunatic is not taken to imply absence of feeling. No more would the inference be warranted in reference to ordinary physical forces.

But some may think that actions even of the erratic kind are far more indicative of the presence of feeling than those that can be predicted and calculated with mathematical precision. The latter are so evidently subject to law that it seems to reduce feeling to the position of a "slave to blind forces" if we regard ordinary physical forces to be accompanied by feeling. This kind of reasoning I imagine would not have much weight with those philosophers who have come to the conclusion that all animal action is automatic; the action of mankind not excepted. And the objection is clearly not a logical one, for the presence of law and order in the connections of feeling with action cannot be shown to be either inconceivable or inconsistent with psychological facts. On the contrary, the regularity with which like feelings are observed to be followed by like actions is so constant that the actions not only of men, but of all animals, are generally calculated and predicted therefrom. If, then, the connection of all "feeling-prompted actions" with law is recognised, both by philosophers and by people in daily life who have no interest in psychological theories, what argument can be drawn therefrom against the connection of feeling with the orderly operation of physical processes? Clearly none. If order and law be observed in the one case, why not in the other?

But this orderly operation of physiological activity has been supposed to go on sometimes in the *absence* of feeling. The incessant action of the heart and its vessels, that of the organs of digestion, and many if not most of our "involuntary" actions, are alleged to be unattended by feeling. In answer to this I should say that it is a question whether the thing which is alleged to occupy no share of our attention should not be described as simply occupying our attention *least*. The fact, as has been pointed out by some writers, that, when the heart and digestive organs are in any diseased or abnormal condition, sensations arise forcibly detaining the attention, proves that what may thus command at occasions much attention may continually be a factor in our feelings, though intellectually we take no note of it. What I mean by this will be best illustrated by dealing with a class of alleged facts of the same kind comprehended under the heads of "feelingless automatic action" or "unconscious cerebration."

Nothing is more common as a fact of experience than our forgetting that we used certain words in conversation or debate. We cannot believe now that we said so-and-so; or, admitting that we may, we explain it as a *lapsis linguæ*, as if due to the mere machinery of vocal utterance unattended by consciousness. Then, again, we sometimes lay past an object, as the banker cited by Dr. Carpenter laid by his safe-key, and forget where we have laid it. Or we may often do something very different from what we originally intended, like the gentleman, again mentioned by Carpenter, who went up-stairs to dress for dinner and "unconsciously" undressed and went to bed.

The statement that we do anything unconsciously, in the sense of *not associating with it all the circumstances* that an ordinarily rational being will usually include, is quite a truism; but the absence of the usual compounding of the separate cognitions of surrounding circumstances with our cognitions of the act we are performing is one thing to admit, and the entire absence of consciousness or feeling in connection with any act is quite another. The former is what we might expect from the complex character of the nervous system as a compound of feeling organs, which complexity renders it liable to operate in parts more or less in isolation from each other. Recollection means a feeble revival of the excitement originally made in the nerves by that object or set of circumstances which we are said to remember. When we fail to remember it the revival does not take place, because the original excitement was *not* made in nerves directly connected with those nerves which are the seat of our mental state when we are trying or wishing to remember. This is proved by our recollecting unexpectedly (after giving it up in despair) in *a different mental* connection—that is, in connection with nerves having a different physical connection. Then are we convinced that it is possible that what we supposed was not attended by feeling or consciousness was really so attended after all.

The mere fact that feeling organs may not operate in connection with one another is no proof that they do not feel individually. In any case the statement as to the absence of feeling in any organ is on a par with the statement that a given organ does feel: neither statement can claim to be supported by absolute proof. The question, therefore, is resolved into one of consistency with known facts, and I have endeavoured to show that the facts cited under the terms "unconscious cerebration" and "feelingless reflex action" are explainable quite consistently with the supposition that all action is prompted by antecedent feeling, whether or not the feelings concerned have connection with other feelings in neighbouring organs.

At this point of the discussion I am reminded that it is incumbent on me to explain consistently with my hypothesis the conditions falling under the terms sleep, swoon, coma, death, and the inorganic state. In this connection I need hardly remark that it is equally necessary to both hypotheses—that of concomitance and alternation alike—to admit that dynamical equilibrium in an organ presupposes the absence of what we ordinarily call feeling; for it is the disturbance of this equilibrium that creates molecular vibration or nervous action—what is ordinarily deemed a mere train of physical sequences. The hypothesis of alternation is quite consistent with unconsciousness where dynamical equilibrium exists, for where no energy is being *received* no feeling can, by the hypothesis, exist. It is impossible to say with absolute certainty that in sleep, coma, or swoon there is total unconsciousness; but even if there is an entire absence of feeling, it is doubtless due to the supervention of a state of equilibrium in the grouped organs of feeling—the nerves. This state of equilibrium may be considered as arising from the withdrawal of the blood-supply which seems necessary to preserve the semi-fluid condition of the "axis-cylinders" or true nerves—a condition in which doubtless vibration can alone arise from the ordinary exciting causes, and therefore a condition indispensable to the presence of feeling. The withdrawal of the blood from the nervous system seems to be the true cause, for it has been observed that sensibility appears to diminish as the circulation is lowered, especially when it is withdrawn from the higher organs of feeling, namely, the co-ordinating and generalising organs that make up the brain.

Death would seem to bring about the perfect condition of equilibrium of the complex system of feeling and acting organs and the supervention of the inorganic state.

Inorganic matter, strictly defined, is a condition of matter in which feeling and acting organs are not arranged to operate in connected series or groups. Now, although all elements that receive energy must by the hypothesis feel and act somewhat like nerves *individually*, yet, inasmuch as these feelings have no grouping organ to connect them, in the case of inorganic matter they must feel as individuals. And since individual feelings are not feelings of feelings, so to speak, from the want of a grouping organ, they are therefore destitute of intelligence (sense of difference), which can alone arise in a grouping organ—an organ connected with several organs so as to feel their different states, or to have their different states communicated to it or combined with its own. But the condition of the inorganic world, commonly so-called, may be the seat of organisation of the kind necessary to intelligence in a degree of which we have never yet dreamt, just as the tissues of some Medusæ, destitute of nerves in the ordinary sense of the term, have been discovered by Mr. Romanes to be the seat of virtual nerves or lines of organised molecular activity. This, however, is for future investigation. Possibly, by the aid of the microphone and extensive research, much truth may yet be gained to connect the so-called inorganic world with what is at present called organic. Life will then be regarded as pervading all Nature, and a Cosmic Philosophy will become possible.

The most formidable objections, however, which the hypothesis of alternation has to answer are objections arising from the custom of regarding the *mental* condition of the thinking and feeling substance, matter, as something quite unique, and opposed to every other condition of that substance.

Mind has been defined by one representative of this mode of thought, namely, Professor Bain, as "opposed to the external world." He says—"The External or Object World is distinguished by the property called Extension. The Internal or the Subject World is our experience of everything *not extended*. A tree which possesses extension is a part of the object world; a pleasure, a volition, a thought, are facts of the subject world." The same writer again says—"We may have a simple *name* for the whole phenomena of mind, as 'The Subject,' 'The Unextended.'"

Now, since Feeling is the general term for all phases of consciousness, for all mental states, Feeling is therefore regarded as Unextended. Mr. Bain does not, in defining mind, say in so many words that the object world includes not only matter and space, but *energy*. Yet he speaks of "resisting matter" and "unresisting empty space," which implies that energy is included under the term "Object." Besides, although he speaks of feelings prompting actions all through his elaborate works on Mind, he speaks of the physical sequences as running side by side with the mental sequences, and of the feelings or mental states as being concomitant with the physical processes in nervous action. It is clear, therefore, that he does not mean to exclude energy from his definition of the object world.

The late Mr. Spalding, also a representative of the same school, evidently included energy along with matter under the term "physical' as opposed to the mental, "the unextended." "The physical and mental," he said, "stand over against each other, a fundamental duality of being which no effort of thought has been able to transcend."

Now, if Feeling be unextended in the exactly opposite sense that energy has extension (namely, of extension in time), feeling will certainly never merge into or alternate with energy, for energy has extension in time. But if it be true that energy has time-extension it is equally true that feeling has time-extension. By energy having extension in time, we mean that it endures in its action for a longer or shorter period. We must apply the same quality to feeling it is evident, for feeling endures also in the same sense as energy. If by applying the term "extension" to energy is meant merely the mental association with our idea of energy of an idea of time-extension, and not that energy actually endures, the same subtle distinction must be applied to feeling, namely, that feeling as extended in time should not be spoken of as a reality, but merely our mental association with the conception in our minds of feeling of an idea of extension is all that we mean when we describe a feeling as extended in time. In this very subtle but hazy language there will be found after all no real contrast between Feeling and Energy, for both must be regarded as having extension in time in exactly the same sense. If we admit, as we must, that Feeling has extension in time, there appears to be no necessity for any longer regarding it as something totally unlike anything else in Nature.

Further, if we institute a comparison between feeling and energy, I think we shall find that they are wonderfully alike. We cannot speak of the length, breadth, or volume of energy or of feeling, and therefore neither can be said to be possessed of space-extension. But we say of energy or feeling equally " it is here, it is there " in an extended body, and therefore, though these affections of matter have no space-extension in themselves they are both *related* to that which has space-extension (as well as time-extension), viz., matter. But their likeness is still more confirmed when we remark that they possess in common other characteristics which we do not observe in the mere substance-matter. We speak of energy or feeling as being " intense or violent, weak or dull," both being qualified as manifesting *degrees* or *quantity*.

Can these strong features of resemblance between feeling and energy justify the ascription of uniqueness to feeling or its opposability to all that is termed physical ? So long as the term physical includes energy it is evident that no such contrast can be legitimately drawn. If the term " physical " merely comprehended the substance-matter it would be legitimate to contrast feeling with matter in the same way that a property is distinguished from that in which the property inheres. But energy, being an affection of matter also, could in the same way be distinguished from the substance wherein it was manifested. There is, indeed, no sense I am aware of in which Feeling can be contrasted with Matter that is not equally applicable to energy.

But if feeling and energy are, after all, so very similar that we apply almost invariably the same language to each, I am at a loss to see wherein they should be considered so dissimilar as that the hypothesis of alternation I have advanced should be deemed untenable. The language of experience describes these two affections as differing only in the sense of receiving and giving. We say " I feel, I act ; I receive an influence, I give forth an influence ; I am passive, I am active." When we use such language we are describing two states of the same being ; subjecting these states to the only contrasts of which they are capable. My experience tells me of no other difference between feeling and energy than that the former is the aroused, awakened, affected ; the latter the arousing, awakening, affecting—phase of my being.

Having met the objection of the alleged total dissimilarity between the mental state, feeling, and the physical state, energy, and endeavoured, as far as my language and logic can, to repel the allegation, I must now deal with the next objection likely to be urged against the hypothesis of alternation.

It is objected that, even admitting the similarity between feeling and energy to be sufficient to satisfy a theory of alternation, *there is yet wanted a locus in time* wherein the supposed alternation may transpire. The "physical processes concerned in nervous action," say the objectors, "are complete in themselves without the intervention of mental states. The dynamical activity of an excited nerve is believed to be perfectly unbroken from first to last. No interval of time, however brief, is regarded as possible during which the dynamical state is suspended. Now it appears to me there are innumerable intervals during the action of a nerve when the dynamical state, strictly so-called, must be suspended, though this may to physicists sound new, strange, and absurd.

Nervous action has been described as molecular vibration, or the to-and-fro movement of the minute constituents of the axis-cylinder or nerve-proper. The rapidity of transit of a nervous impulse is claimed to have been measured, and a very humble speed it is. But even if it were as swift as the swiftest known force, it would still be a period of time. And just as the time-period of the motion of the first molecule of the nerve is distinctly separated from the time-period of the last molecule's motion by the times of motion of each of the intervening molecules, so there is a period during which each molecule is in the attitude of receiving energy from its preceding neighbour *distinct from the period* during which it is in the act of imparting its energy to the succeeding molecule. Receiving and imparting are not simultaneous in occurrence, else time would be blotted out, the last molecule would move simultaneously with the first. The very fact that time is an element at all involves the admission that the whole time of vibration is divisible into the individual times of the vibrating elements, and the times of each of these into times of receiving and giving energy. And since the imparting stage is the only one of the two conditions of a vibrating molecule which can be strictly called dynamical, active, energising, it follows that the dynamical periods of the entire nerve are interrupted by periods that cannot be strictly called dynamical, since they are periods of passivity; periods of being affected, aroused, awakened; periods during which a condition exists which exactly corresponds with the condition we call feeling, or the mental state. These periods furnish, then, the desired *locus* in time, in which feeling may transpire and alternate with energising affections.

I am aware that a physicist might reply that the reason why nervous vibration—like all other vibration—involves the element of time is that space has to be travelled over, and that all the period between the receiving of energy by the first molecule and imparting it to the second molecule is occupied by traversing the distance between them, and that if such distance did not exist the motion of the first and last molecules would be simultaneous, and the condition of all the molecules would be a dynamical one. Therefore, that the condition of a molecule separated by distance from a second molecule is simply a dynamical one from the instant of its first receiving energy. Doubtless, I reply, if we obliterate space we obliterate time, and if we in theory blot these out there will surely not be much power left in us to imagine either feeling or energy! But we cannot conceive of energy without supposing something upon which energy is to be exerted. This implies plurality of objects or parts of matter, and this again implies parts of space and time. That the time of receiving energy by a molecule is not identical with that of its imparting the received energy is proved even by the law of inertia, which will admit of no beginning of movement till the requisite energy is all received. Now there must be a period, in the case of every molecule about to take part in vibration, during which it is in the condition of receiving its quota of energy necessary to overcome its inertia *prior to its beginning to move*, or impart the energy being communicated to it. This is the necessary *locus in time* where feeling may come in as incipient energy, or an affection of matter resembling energy, yet differing from it by its characteristic feature, *passivity*.

Thus far the hypothesis of alternation between mental and physical, or feeling and energising, affections of matter has its difficulties explained away.

In conclusion, it may be remarked that while the hypothesis links feeling with energy in a causal relation, yet no harm will accrue to physical science by its acceptance; for the dynamical links, though deemed alternating with links of feeling, need not to be reckoned in any problems of physics as affected by the mental states intervening, for the mental are the results of the physical, and the physical of the mental, in such a manner that no energy is lost, and all is exactly as if the dynamical sequences were uninterrupted. Again, no discrepancy need arise in psychological science by the intervention of dynamical with mental affections, for the actions alternating with feeling are those only which arise in the nerves,—or organs of feeling and action,—and not in the muscles, whose action, though doubtless preceded by feeling in themselves, are no more to be considered as exponents of the nervous action than the explosion of a powder-magazine is an exponent of the energy present in the ignited match. Even the hypothesis of concomitance is by the new hypothesis accounted for, by reason of the brevity of the period of transformation of feeling into energy, which practically makes the two series of affections have the appearance of concomitance.

The new hypothesis sheds a new light upon Feeling and Energy alike ; for if it be correct, then there is no " enduring " feeling strictly so-called. What is called an " enduring " feeling is really a succession of inconceivably brief mental states interrupted by dynamical states, as a musical note is the repetition of sound-impulses interrupted by periods of silence.

NOTICES OF BOOKS.

The Lake Dwellings of Switzerland and other parts of Europe. By Dr. FERDINAND KELLER. Translated and Arranged by JOHN EDWARD LEE, F.S.A., F.G.S. In Two Volumes. London: Longmans and Co.

NATURE sometimes seems impatient at man's slow progress, and as if she desired to assist him in his researches. A great storm sweeps away the sand and shingle from the coast, and bares to the eye of the geologist buried forests and fossiliferous strata containing some of the missing links in the great chain of life. At another time the waves cast up on the shore some curious inhabitant of the deep, before unknown to Science. And so, in Switzerland, the pile dwellings might long have remained unexplored if it had not been for the great drought and extended frost of the winter of 1853-4, which caused the rivers to shrink to their smallest compass, and the level of the lakes to fall lower than is ever recorded in history, before this time.

Then were exposed, in a way that could not but arouse attention, the remains of habitations some of which are probably as old as the mysterious dolmens, and were used by the earliest of the neolithic immigrants. Notwithstanding their vast antiquity. the sediment at the bottoms of the quiet lakes had preserved not only the piles themselves, and articles of wood, stone, and pottery, but the provisions that had been stored for winter use, linen cloth, and fishermen's lines and nets. The great opportunity thus afforded for examining these remains was not neglected, and under the enthusiastic yet cautious leadership of Dr. Ferdinand Keller the explorations have been zealously carried on from that time to the present day.

A translation of Dr. Keller's earlier reports was published by Mr. Lee in 1866, and has been the principal work of reference for English students. Since then a great many additional discoveries have been made, not only in the lakes, but in many of the peat-bogs which have been proved to be filled-up lake-basins. The area over which the lake dwellers lived is shown to have extended far beyond Switzerland, into Italy, Austria, and Hungary. Now, more than ten years after the first edition, Mr. Lee lays us under a fresh obligation by a second, in which it is scarcely too much to say that the additional information is nearly equal to the whole contained in the first, Modestly terming it a translation of Dr. Keller's memoirs, Mr. Lee gives us what is in reality the work rather of a student who, wishing to ascribe all the honour to the master under whom he had studied, lays his own labours at his feet. A mere translation it is not. The antiquities themselves have been carefully studied by Mr. Lee, who brings, from a variety of sources, information to bear upon them, and appears to have neglected nothing that he thought might afford help in their elucidation. He has done for the Lake Dwellers of Switzerland and Italy what Prof. Rupert Jones --in his edition of Messrs. Lartet and Christy's "Reliquiæ Aquitanicæ"—has done for the Cave Dwellers of France; and it is instructive and suggestive to study the two works together, and to note the few points of resemblance and the many of contrast in the two peoples who occupied Central Europe at far removed periods, and in that area appear never to have come in contact. That the lacustrine people did not differ in race from those who lived on the dry land is proved by the fact that the implements of stone and bronze found in graves and tumuli, and on the surface in parts where there are no lakes, are the same in material, form, and ornamentation as those obtained from the pile-dwellings. Articles of bronze, stone, horn, bone, and earthenware, exactly similar to those used by the lake dwellers, were found in a settlement on the mainland at Ebersberg.

The people living on the land chose, for their settlements, positions on the sides and crests of hills that offered facilities for defence. These were hill-forts, and so the settlements in the lakes may be considered water-forts; the object in both cases being safety against the attacks of neighbouring tribes. In the early history of mankind, as well as amongst the lowest of existing races, every small community was or is at enmity with neighbouring ones; and so amongst these earliest neolithic settlers of Switzerland, some entrenched themselves on the hills, others in the lakes as far from the shore as they could drive their piles, and were the same people adapting themselves to the necessities of their surroundings.

The lake settlements were formed by first driving piles into the sand or clay in shallow parts; sometimes the piles were strengthened by cross timbers mortised into them, or by stones being brought from the land and heaped up around them. The tops of the piles were brought to the same level, and on them was formed a platform of small stems of trees, covered with mud, loam, and gravel. On this platform the huts of the settlers were reared. The walls consisted of upright posts, between which a wattle-work of small branches was interwoven, and the whole filled up and covered with clay. The roof was thatched with straw or reeds.

A narrow bridge or platform, built on piles, connected the settlement with the shore. It might have been safer to have done without this, and to have kept up the communication by means of canoes only; but the settlers stalled their cattle in the lake dwellings, and it would be necessary to have a more permanent connection with the land to get them to and from their pastures.

Many of the settlements were destroyed by fire, to which, from the combustible character of their materials, they must have been particularly liable. It is to the occurrence of these fires that we owe the preservation of many of the articles that have been obtained from the bottoms of the lakes. The whole of the provisions stored up for winter use have in some cases been precipitated into the water by the giving way of the burning piles. Rare implements of jade and personal ornaments have at these times been lost and covered up amongst the *débris*. Even loaves of bread and fruits have been preserved through having been charred before falling into the water.

The oldest of the lake dwellings appear to date back as far as the earliest traces we have of neolithic man in Central Europe. In these the implements are of stone, bone, and horn, and the pottery is rude and entirely hand-made. At this time the lake dwellers subsisted largely upon wild animals and fruits, though domestic animals and cultivated plants were not unknown. Weaving linen cloth was practised in the oldest settlements, and hanks of unspun flax and thread, cord, nets, and cloth of the same material have been found.

Up from the rudest stage of the stone age there is a regular progression to be observed. The bronze age comes in gradually; at first a bronze celt being found amongst the stone ones; then stone-implements become scarce, and those of bronze common and of improved form and ornamentation. Throughout, the evidence points to the gradual progression in civilisation of the same people, influenced, doubtless, by others in a more advanced stage living to the south and east, but without any sign of the sudden intrusion of a new race bringing with them new weapons and customs. Prof. Desor has also shown that the form of skull prevalent in the stone age continued through the bronze and iron ages, continually increasing in size, and showing a broader and higher forehead. It is the same type of skull that prevails in the Swiss valleys at the present day, showing that the direct descendants of the builders of the pile dwellings still live around the shores of the lakes.

Some of the breeds of domestic cattle they kept and of the varieties of grain they cultivated have also survived to our time, though greatly improved. The "marsh cow" of the lake dwellings is represented by the common Swiss "brown cow," and some of our varieties of wheat are, according to Dr. Heer, the descendants of those grown by the lake dwellers. None of the animals known in these ancient times have become extinct, though the domestic breeds have been improved and the wild species have in some cases deteriorated,—probably, in the latter case, through the curtailment of their feeding-grounds by man.

The remains of the common fowl have not been met with in the lake dwellings, and this gives us an approximate date for the latest of those belonging to the bronze age, as it is not mentioned by Homer, and is first referred to by Greek authors about 400 B.C. In the time of Pericles it was known as the Persian bird, from which we may gather it was brought from that country to Europe. It was unknown to the ancient Egyptians, and is not mentioned in the Old Testament. It had, however, spread into Western Europe before the Christian era, as it appears on the earliest of the Gallic coins, and was found by Julius Cæsar in Britain. Probably, therefore, we may place the date of the latest of the settlements of the bronze age at from 2400 to 3000 years ago.

It is often stated that Europe was peopled directly from Asia, but the relics found in the earliest of the lake dwellings do not favour this conclusion, but rather show an intimate relation with Egypt and the shores of the Mediterranean. Rye, which was cultivated in the east in the bronze age, is not found in the lake dwellings, and it was equally unknown to the ancient Egyptians. The cereals cultivated by the lacustrine people were all Egyptian or Italian. The eastern hemp was not used by either the lake dwellers or the ancient Egyptians; flax was largely grown, spun, and woven by both. Even the weeds introduced with the seed flax and corn point to the southern origin of the people. Dr. Heer finds the seeds of the Cretan catchfly in the remains from the lake dwellings. This plant does not now grow in Switzerland or Germany, but is found everywhere in the flax fields of the countries bordering the Mediterranean. The corn bluebottle, a native of Sicily, also grew in the fields of the lake dwellers; and the water chestnut had probably been introduced from Italy.

The influence of Egypt is shown in another way. A number of large crescent-shaped objects, made of pottery and wood, have been found amongst the remains of the lake dwellings. At first some thought these were indications of the worship of the moon, others that they were rude representations of the heads of bulls. Similar crescents have, however, been discovered in abundance amongst the Egyptian antiquities, and there is no doubt but that they were used as pillows by a people who wore their hair in the form of thick plaited head-dresses. The sleeper rested his neck in the hollow of the crescent, so as to prevent his carefully prepared head-dress from being disarranged. That the lake dwellers really wore their hair in thick plaited masses is proved by the discovery of very many long hair-pins. Even at the present day similar pillows are used by different tribes in Africa and Polynesia, who fasten up their hair in thick masses with pins 9 inches long.

The only fact pointing to a communication with the east is the presence of celts made of jade. This stone must have been brought from the east, and the advocates of the Oriental origin of the first settlers in Europe have supposed that they brought the implements made of it with them. But in that case, either the first settlers must have come laden with these implements to supply both themselves and their descendants, or the latter must have obtained them by barter from the east. In one of the latest settlements of the stone age—at Gerlafingen, in the Lake of Bienne—some of the finest jade-implements have been found. Two chisels of pure copper and some bronze celts of primitive type indicate that the people of this settlement lived at the close of the stone age. As they have been shown to be the descendants of the earlier lake dwellers, they could only have obtained these articles through traders, and, if they did so, their forefathers might have done the same. It is known that in ancient America copper articles found their way from the northern lakes, passing from tribe to tribe far to the south, whilst, on the other hand, the obsidian implements of Mexico were carried northward. The system of barter exists now amongst tribes more rude and savage than the Swiss lake dwellers. In those parts of Asia where jade occurs amongst the rocks, the ancient inhabitants would soon discover that they possessed an article of export for which they could obtain whatever they might wish in exchange from the nations of the West. And the latter, in the linen cloth they fabricated, had one article at least that they might barter for the coveted jade implements.

About the origin of the still earlier people that lived in Switzerland and other parts of Europe long before the lake dwellers —the people of the reindeer period—we know absolutely nothing. We know that they lived in Europe along with extinct species of elephant and rhinoceros, and many other animals not now found in Europe, and that in the latter part of their time the reindeer abounded throughout Central Europe and as far south as the Pyrenees. We know, too, that the climate was much colder, and that the musk sheep and other arctic animals lived in the South of France. But where palæolithic man came from there is as yet no evidence to show.

His outgoing is almost as much shrouded in mystery as his incoming. He simply disappears, and with him vanish the great beasts amongst which he lived. There is not a single point of connection between the latest of the palæolithic and the earliest of the neolithic tribes. Both, it is true, used stone-implements, but they are so essentially distinct in type that they can be recognised at a glance, and there are no intermediate forms showing the development of the one from the other. We have seen that there has been a regular, quiet, and gradual progression of the people of the stone age of the lake dwellings, through the bronze and iron periods, up to the present time. The people themselves are still represented, their arts remain, and every species of animal and plant they knew still exists.

But when we go back beyond the age of polished stone-implements there is a complete break,—a period unrepresented, excepting by physical changes in the appearance of the country, and by the deposition of clays and gravels, beneath which the relics of the earlier people and the remains of the extinct animals lie buried.

The evidence of the completeness of the break, and of the importance of the interval that separate palæolithic and neolithic man, is to be found in these volumes, and in Prof. Rupert Jones's edition of the "Reliquiæ Aquitanicæ." The reindeer is absent from the lake dwellings. The dog and the sheep are found in the earliest settlements, and both were unknown to the cave dwellers. The latter knew nothing of agriculture, of stock-keeping, or of weaving, and yet in the figures of the animals that they have left behind them—engraved on horn and ivory—they show a phase of art culture to which the lake dwellers never attained.

Had these different races ever come in contact as enemies, we ought to have found amongst the remains of the pile settlements some trophies taken from the people they displaced. Tusks of the brown bear and of the wild boar, perforated so that they might be worn as ornaments, are not uncommon, and show that they would have carefully preserved the proofs of their victories over nobler foes if they had ever encountered them. But there are none, and this—considered in connection with the other facts mentioned, and with the physical evidence that a long time intervened between the two peoples and faunas—indicates that they never met in Central Europe. As the evidence now stands it seems to warrant the conclusion that palæolithic man and the extinct animals were destroyed in Northern and Central Europe by some physical catastrophe, and that, after a long interval, neolithic man migrated northward from the shores of the Mediterranean and from the Iberian peninsula, and found the land unoccupied excepting by species of wild animals that had escaped extirpation, and which, notwithstanding the persecution to which they have been subjected since, still exist amongst us.

The questions touched upon in this review are only a few of those suggested in reading through Mr. Lee's most valuable work. It should be in the hands of every student of anthropology. The letterpress extends to 725 pages, and there are no less than 206 plates, with admirable figures of the lake dwellings and of the multitudinous objects found in them.

Acadian Geology. The Geological Structure, Organic Remains,
and Mineral Resources of Nova Scotia, New Brunswick,
and Prince Edward's Island. By J. W. Dawson, F.R.S.
London: Macmillan and Co.

We have here the pleasure of meeting with Principal Dawson not as the scarcely-candid anti-Evolutionist zealot, prone to "high-falutin" language, and to bringing against opponents charges of intellectual and even moral obliquity, but as the earnest and successful worker in geological research. In this field he has made his mark, and, even though the evidence in favour of the organic nature of his *Eozoon* is not increasing, he has an indisputable claim to the gratitude of the scientific world.

The work before us is full of important observations, and brings us face to face with some most interesting questions, at which we regret that want of space allows us to take merely a passing glance.

Is Nova Scotia, with the adjacent parts of the Dominion, subsiding? We have heard it maintained that the harbour of Halifax was being gradually upheaved, and must at no very distant date become useless. But Prof. Cook, in a paper here referred to, gives a summary of indications of modern subsidence observed on the coasts of New England and New York, and estimates the average rate of sinking, as now in progress, at 2 feet per century. In some parts of Nova Scotia the tides are found to rise higher than formerly, and the reclaimed marshes—an exceedingly fruitful tract—are exposed to some peril.

A question of wider interest is the existence of a former Glacial epoch as the cause of certain geological phenomena observed both in Europe and America. The existence of such a period "when the whole of the northern parts of Europe and North America are imagined to have been covered with glaciers, or rather with an universal glacier like that of Greenland, but on an enormously larger scale," the author considers improbable, whether considered "in a mechanical, meteorological, or geological point of view." He contends that floating ice and the Arctic currents have been the grand agents in the distribution of erratic blocks, and in the production of scratched and polished rock-surfaces. He suggests that, as the country was gradually subsiding, blocks imbedded in ice were driven against the base of the hills. As the land continued to sink, the ice-fields of successive years gradually pushed them higher, until the summits of the hills were submerged so deeply that the ice could no longer take up the blocks. Concerning Prof. Frankland's hypothesis of a higher temperature of the sea conjointly with a lower temperature of the land, Dr. Dawson remarks that such an "inversion of the usual state of things is unwarranted by the doctrine of the secular cooling of the earth; it is contradicted by the fossils of the period, which show that the seas were colder than at present, and if it existed it could not produce the effects required, unless a preternatural arrest were at the same time laid on the winds which spread the temperature of the sea over the land."

The author has "failed to find, even in our higher mountains, any distinct signs of glacier action, though the action of the ocean-breakers is visible almost to their summits; and though I have observed in Canada and Nova Scotia many old sea-beaches, gravel-ridges, and lake-margins, I have seen nothing that could fairly be regarded as the work of glaciers." He holds that while a great and marked climatic revolution has occurred in Europe, the evidences of such a change are very much slighter in America, where the causes of the coldness of the post-pliocene seas to some extent still remain. The author, in the following passage, admits substantially the existence of a former epoch of intense cold:—"In the Tertiary era there was much dry land in the northern hemisphere, and multitudes of large animals now extinct inhabited it, apparently under a climate milder than at present. Great changes, however, took place in the relative positions of land and water, inducing very important changes of climate, *which finally became of an almost arctic character over all the present temperate regions.* The greater part of Northern Europe and Asia appear to have subsided beneath the waters of the boulder-bearing semi-arctic ocean." It would almost seem that Dr. Dawson attributes the climatic deterioration to a decrease of land in the higher latitudes, taking thus a view opposite to that of Lyell. We fear that the very foundations for a thorough settlement of the glacial question stand in need of careful revision.

In closing this interesting and instructive volume we can only pronounce it indispensable to every geologist.

A Manual of Zoology for the Use of Students. By H. ALLEYNE NICHOLSON, M.D., D.Sc., &c. Fifth Edition, Revised and Enlarged. Edinburgh and London: W. Blackwood and Sons.

WHEN a scientific work has, like the volume before us, reached its fifth edition, and has met with the general approval of the highest authorities, the task of the critic is greatly simplified. There are indeed points on which issue might be joined, and there also topics on which somewhat fuller information might be desirable. Thus, we cannot pronounce the common British centipede harmless, having received from one a bite decidedly more severe than the sting of the wasp. The bite of the viper, as far as we are aware, proves mortal, not merely to children and debilitated persons, but to about 20 per cent of the sufferers. Nor, in presence of the facts recorded by Dr. Coues, can we pronounce the skunk, "when unmolested, perfectly harmless." Passing from facts to matters of opinion, we must protest against the exaggerated view taken by the author of the interval between man and the anthropoid apes. Not satisfied with giving to the former, as did Cuvier, the rank of a separate order, Dr. Nicholson even questions whether the human species "should not have the value of a distinct sub-kingdom, whilst there can be little hesitation in giving Man, at any rate, a class to himself." As a deficiency, we may point out that the information given concerning the sense-organs of the vertebrate animals is somewhat meagre.

But passing over what to us, at least, appear as shortcomings, we hold that it would be difficult indeed to find a work which gives, in so brief a compass, so luminous and philosophical a view of the whole animal kingdom. To any earnest student entering upon the science of Biology the "General Introduction" alone must be a boon of the highest order.

A Key to Organic Materia Medica, &c. By Dr. JOHN MUTER, M.A., F.C.S., Director of the South London School of Pharmacy. Second Edition. London: Simpkin and Marshall. 1878.

THE book before us is another proof that the training of pharmacists is becoming daily more thoroughly scientific. The first edition of this work appeared about four years ago, and was intended principally as a guide for the students frequenting Dr. Muter's School of Pharmacy, at Kennington Cross. The present issue, however, has been so enlarged and improved that it has become available for study to all classes of pharmaceutical students, whether under Dr. Muter's tuition or not. The products described are given in their botanical and zoological order, beginning with Aconite and ending with Castoreum; the source, method of gathering, description, uses, chemistry, and pharmacology of each being described in succinct but sufficiently detailed terms. Dr. Muter is evidently alive to the fact that most pharmaceutical students of necessity consume large quantities of midnight oil, for his book is printed in large clear type. In the Appendix is given an account of Dr. Muter's method of mounting, observing, measuring, and classifying the starches, which will be found equally useful to the vegetable physiologist and the pharmacist. The Index is a model of its kind, and is due to the pen of Mr. Joseph Ince, who deserves for it a vote of thanks from the Index Society.

Memoirs of the Geological Survey of India. Palæontologia Indica, being Figures and Descriptions of the Organic Remains procured during the progress of the Geological Survey of India. Indian Tertiary and Post Tertiary Vertebrata. Vol. i., 2. Ser. x., 2.—Molar Teeth and other remains of Mammalia, by R. LYDEKKER, B.A. Calcutta: Geological Survey Office. London: Trübner and Co.

THE remains here described and figured belong to *Rhinoceros palæindicus*, *R. sivalensis*, *R. platyrhinus*, *R. iravadicus* (a new species), *R. planidens*, and *Acerotherium Perimense.* If we include both extinct and living forms the total number of species of Rhinocerotes found in South-Eastern Asia is fifteen, whilst there is evidence of three further species from Burma, Attock, and Sind respectively. Of ruminants we find a description of *Vishnutherium iravadicum*, or at least of its teeth and a fragment of its jaw. Till the entire skull is obtained it will be difficult to say whether this extinct form was most nearly allied to *Sivatherium* or to *Camelopardalis.* Of this latter genus there is a figure and description of the remains of *C. sivalensis.* Next follow *Bramatherium Perimense*, *Camelus sivalensis* (a specific name which we fear occurs too often), and *Dorcatherium majus* and *minus* (two new species). *Cervus latidens* was the largest of the Cervidæ of Siwalik, its teeth equalling in size those of the Irish elk. The other Cervidæ are *C. triplidens* and *C. simplicidens.* The teeth of *Listriodon pentapotamiæ* might be mistaken for the lower molars of *Tapirus* were it not for their square form. *Dinotherium pentapotamiæ* is distinguished from the European species by its much smaller size. The teeth of *Sanitherium Schlagintweitii* are distinguished from those of *Sus* by the greater simplicity and distinctness of the main tubercles; by the hinder lobe being relatively larger and taller; and by the plane of wear being more oblique. *Tetraconodon magnum*, a hippopotamoid animal, is characterised by the abnormal development of its pre-molar teeth, which are considerably larger than the two molars.

The edentate animal, *Manis Sindiensis*, had the same general organisation as the living species. *Amphycyon palæindicus*, a carnivorous animal approximating to the ursine group, was nearly as large as the polar bear.

Ser. ii., 2, contains descriptions and illustrations of a number of fossil plants discovered in the Jurassic (Lias) formation of the Rajmahal Hills. This flora has a mesozoic character; it is much more numerous than that from Kach, both in species and specimens. Its chief types of the Rajmahal flora are in the class of Cycadeaceæ and some genera of ferns.

Descriptive Catalogue of Photographs of North-American Indians. By W. H. Jackson. Washington: Government Printing-Office.

The collection of photographs of members of different North-American tribes, formed under the auspices of the United States Geological Survey of the Territories, is an ethnological monument of peculiar value. It embraces over one thousand negatives, representing no fewer than twenty-five distinct tribes. Of these not a few are in process of extinction, whilst the rest are undergoing a process of intermixture with each other, and with the different nationalities of European or African origin who have overspread the western hemisphere. Hence a collection like the one in question is necessarily unique, and if it should be allowed to perish could never be reproduced. The catalogue is arranged ethnologically, and includes a short history of each tribe.

The Scientific Basis of Music. By W. H. Stone, M.A., M.B. Oxon, F.R.C.P., Lecturer on Physics at St. Thomas's Hospital, Vice-President of the Physical Society. 71 pp. London: Novello, Ewer, and Co.

This little book is one of a series of "Music Primers" issued by the above enterprising publishers. Although intended as manuals of instruction for beginners, they are so carefully written, and so much above the ordinary run of books—good, bad, and indifferent—which we meet with as tutors and instructors in various branches of music, that they can be read with interest even by advanced students, and, as far as they go, are worthy to take rank with some of the admirable works published under the auspices of the Paris Conservatoire. Dr. Stone's work treats on a department hitherto much neglected by the musical student: the author—himself a good practical musician, as well as a physicist—is admirably qualified for the task he has undertaken.

In the first chapter the sources of sound are fully and clearly explained. At page 21 the author mentions the class of *Membranous Reeds,* such as the larynx and the human lip acting on the cupped mouthpiece of a brass instrument: the author defers the consideration of the subject to a future chapter, but careful reading fails to find any further mention of this interesting class of sound-producers; it is to be hoped that this defect may be remedied in a future edition.

The following chapters explain the subjects of Velocity, Reflection, Refraction, Interference, Tonometry; Musical Tone, Harmonics, Consonance, Quality; Concord and Discord, and Resultant Tones.

The last chapter, on Scale and Temperament, gives an account of several contrivances, all more or less elaborate, having for their object the perfect tuning of keyed instruments. It is to be feared that these complex key-boards offer such obstacles to freedom of execution as to be useless for all practical purposes. Dr. Stone's remarks on tuning an orchestra are admirable, and will be read to advantage by every instrumentalist, many of whom, as conductors know too well, are extremely loose in their ideas of accuracy.

A page at the end of the little book is devoted to the bibliography of the subject, and will greatly aid the student in extending his researches.

Studies in Spectrum Analysis. By J. NORMAN LOCKYER, F.R.S. Second Edition. London: C. Kegan Paul and Co., 1, Paternoster Row. 1878.

MR. NORMAN LOCKYER'S present volume forms part of Messrs. Paul's excellent International Scientific Series, and is written for the perusal of the more serious portion of the reading public, as well as for the more purely scientific student. Beginning with the vibrations of a jerked rope, the author explains, in a singularly clear and homely manner, the phenomena connected with wave-motion, the illustrations employed by him being of a nature which will be understood by every observant person. He shows that the principles upon which all undulations are produced —whether they are caused by muscular force, sound, heat, light or electricity—are similar, if not identical, a point which is only too frequently lost sight of by writers and lecturers on popular science. If, by setting forth the relationship between a note in music and a bright line in the spectrum, a thoughtful reader or lecturer can by analogy be made to see how the effects of a change in the rate of the wave-motion of the luminiferous ether as affecting colours are brought about, a great point is gained. We believe Prof. Barrett, in his lectures on the Connection between Light and Sound, was one of the first to break ground in this direction: and the fact that light is nothing more than an excessively fine kind of sound, the effects of which were perceived by the eye instead of the ear, was a revelation to the majority of his hearers. Mr. Lockyer treats of sound, light, heat, and electricity concurrently as being parts of one grand system of wave-motion, and we should be pleased to see his example followed by writers on elementary science who only are too fond of separating these subjects by hard-and-fast lines instead of treating them correlatively.

The present work may be said to be in some sort the complement of Mr. Lockyer's former work on "The Spectroscope and its Variations," seeing that it goes much more deeply into the theoretical part of this subject. The subject of wave-motion having been explained, the author next describes the principal methods of demonstrating spectral phenomena, and the value and use of the photographic camera in their registration.

The fourth chapter, in which atoms and molecules are treated of in relation to the spectroscope, contains a large amount of theoretical speculation of a very interesting character; and the same may be said of the chapter on Dissociation, in which Mr. Lockyer boldly attacks the integrity of certain of the elements, such as calcium and hydrogen. Mr. Lockyer's speculations in this direction are of a most suggestive kind, and ought to form the starting-points for a large amount of investigation. If our present elements are ever to be split up, either theoretically or practically, the spectroscope will have undoubtedly commenced the work.

The account given of Messrs. Lockyer and Roberts's experiments on the quantitative analysis of gold and other alloys show that one of these days the spectroscope will play an important part in the assay of the precious metals. The results given by these experiments are of a very promising nature, and it is only the close attention which Mr. Lockyer has to give to the more important branches of Spectroscopy that prevents him and his colleague from bringing their researches to a thoroughly practical termination. By the present method an assay takes at least two hours, whereas by the use of the spectroscope it might be performed in a few minutes.

The book is well illustrated by eight photo-lithographic plates of spectra and fifty well-executed woodcuts.

Industrial Chemistry. A Manual for Use in Technical Colleges and Schools, and for Manufacturers, &c. Based upon a Translation, partly by Dr. T. D. Barry, of Stohmann and Engler's German Edition of Payen's "Précis de Chimie Industrielle." Edited throughout, and supplemented with Chapters on the Chemistry of the Metals, &c., by B. H. Paul, Ph.D. Illustrated with 668 Engravings on Wood. London: Longmans and Co. 1878.

After a very attentive examination of this, the most recent contribution to the literature of Industrial Chemistry, we must confess that we are utterly puzzled to discover for whose benefit this book has been compiled. Judging from the title-page and preface, it is not only intended for manufacturers and technological students, but for schoolboys and the general public as well. Now, if it is intended for the first-mentioned category of readers, it is at once too copious and too meagre, teaching those things it ought not to teach, and leaving untaught those things it ought to teach; if for the latter, it is again far too detailed in some parts and too superficial in others. The great mistake committed by the whole of the five writers connected with the work has been in endeavouring to teach too many things at once, the consequence being that the book is overloaded with instructions in the rudiments of chemical science, and descriptions of elements and compounds that have nothing to do with Industrial Chemistry. The natural result of this waste of space is that hosts of important compounds, whose names have long been household words even amongst the general public, are either passed over in silence or dismissed in a few lines. Will it be credited that—in a work published in the last quarter of the nineteenth century, and written "for manufacturers, &c."—while page after page is devoted to lengthy descriptions of such rarely seen elements as tellurium, gallium, yttrium, and a dozen others, half a page is given to platinum as an industrial metal, and such everyday products as dynamite and carbolic acid are barely mentioned. Had the space occupied by so much useless and extraneous matter, all of which may be found in any half-crown Manual of Chemistry, been devoted to the real object of the work, we might possibly have had a few pages given to such minor matters as calico-printing, aniline, anthracen and naphthalin dyes, collodion, Esparto grass, pebble powder, oxalic acid and its salts, tannic and pyrogallic acid, picric acid, and other chemical products which are constantly mentioned in the columns of every newspaper.

The way in which the translation is executed and edited leaves much to be desired; but the portions added to Messrs. Stohmann and Engler's adaptation of Payen's "Précis," which are evidently from the practised pen of Dr. Paul, form a strong contrast, in the ease and completeness with which they are written, to the disjointed style of the remainder of the work. The additions, in fact, are the best part of the book, which is more than half again as large as Stohmann and Engler's translation.

The work is ostensibly intended for English use, but in many parts too much favour has been shown to foreign processes to the entire exclusion of the methods of working usually adopted in this country. For instance, although Weldon's chlorine process is fairly described, Spence's improvements in the manufacture of alum and Mactear's method of sulphur recovery are entirely ignored. These are only two out of the many English processes which have been passed over in silence.

Another singular defect, which ought not to exist in a book of this sort, is the almost entire absence of references to fuller accounts of processes than the space at the editors' disposal would allow them to give—a fault that is also to be found in the German and French editions.

It is with great regret that we feel called upon to be so severe upon a work which has appeared with so many well and favourably known names attached to it. Such a book—we mean a manual of technological chemistry in the fullest sense of the term—is much wanted, but it must consist of many volumes, and be written and edited by more than one man. A book on industrial chemistry in a single volume is of but little use to the manufacturer or the technological student. A popular account of the more striking improvements which have lately taken place in chemical manufactures, treated in a lively style, and a plainly written manual of chemistry applied to the arts, for the use of schoolboys who have already gained some knowledge of the science, are both loudly called for, but Drs. Paul and Barry's compilation satisfies none of these requirements. It is also to be regretted that the beautiful copper plate engravings of the French and German editions have been replaced by small woodcuts.

Metals and their Chief Industrial Applications. By CHARLES R. ALDER WRIGHT, D.Sc., &c. London: Macmillan and Co. 1878.

THIS book contains the substance, with several valuable additions, of the course of lectures on this subject delivered by Dr. Wright, last year, at the Royal Institution. The author has the gift of conveying a large amount of practical information in a few words, without, however, falling into the sin of dryness. The lectures are avowedly popular; theory, therefore, receives comparatively little attention. The principles, however, upon which different ores are made to yield up their metals are clearly and succinctly explained. Dr. Wright has brought his information down to the very latest date, and the processes of Siemens, Bessemer, and other modern workers in the field of metallurgy are fully described. As an instance of the author's conscientiousness in this respect it may be mentioned that Allen's nickel process, described in the "Journal of the Society of Arts" of so late a date as last February, is to be found in its proper place.

For a young student desirous of gaining a general insight into metallurgical processes there could hardly be a better manual. The book is well illustrated; and although it seems ungracious to find fault where there is so much to praise, we must remind Dr. Wright that an Index is an essential part of such a work.

Transactions of the Edinburgh Geological Society. Vol. iii., Part 1. Edinburgh. Printed for the Society. 1877.

THIS issue contains a large amount of interesting and valuable matter. There is an Inaugural Address on the "Palæontological Signification of the Migrations of Animals," delivered by Prof. H. A. Nicholson. The speaker treats almost exclusively of Marine Invertebrata, and the following are some of his general conclusions:—Oceanic currents have a great influence on the distribution of marine animals, acting not only upon pelagic species, but also upon the locomotive young of littoral species. Under any circumstances there is a probability that pelagic species will have a wider range than those of littoral habits. Littoral species are generally limited in their distribution by the depth of water near land, by the trend of the land itself, and by the extension of the shore. A species requiring a definite high or low temperature for its existence will spread much farther along a shore running east and west than it can along one having a north and south direction. The conditions which limit the range of littoral species are not stable and permanent, but vary much at different periods in consequence of subsidence or elevation of the land. The distribution of pelagic species is mainly influenced by the surface temperature of the water, but it may be temporarily or permanently affected by winds or currents. Deep-sea forms are usually widely diffused, their range depending chiefly on temperature, and being especially influenced by oceanic currents. Fresh-water Invertebrates are, in the main, governed in their migrations by the same laws as those which influence marine forms. Some marine Invertebrates are probably capable of adapting themselves to a gradual change of the sea in which they live to fresh water.

In a paper by Mr. R. Richardson, on "Phenomena of Weather-Action and Glaciation," we find some interesting observations on that process of erosion which certain so-called sceptics in geology are endeavouring to deny. But the man who can shut his eyes to the phenomena which on a larger or smaller scale are to be witnessed in every mountain-chain will certainly not be convinced by any human testimony. That the glaciation of Switzerland has decreased, and is still decreasing, the author finds abundant proof.

Dr. D. Hahn, in a humorous paper, advocates the introduction of a dual nomenclature in mineralogy, similar to that established in zoology and botany. We regret to notice that certain German names of minerals mentioned are greatly obscured by typographical errors. Dr. Hahn also communicates a paper on the "Phosphorescence of Minerals."

Mr. A. Somervail's communication, "On the Glacial Phenomena of Scotland, with especial reference to the recent works of

Dr. Croll and Mr. James Geikie," agrees in many points with the views put forward by Mr. Mattieu Williams in his "Through Norway with Ladies" (see "Quarterly Journal of Science," vol. vii., p. 537). Mr. Somervail, though by no means questioning the fact of glaciation or of a glacial epoch, entertains doubts concerning the so-called "Interglacial periods," and holds that "the conditions under which the lower drift deposits have been formed were not of that severe and rigorous kind that has of late been so vigorously advocated." He does not find evidence for a vast ice-sheet coming from Scandinavia, and coalescing, upon what is now the bed of the North Sea, with another sheet descending from the Highlands. He remarks that many of the supposed interglacial beds, which, according to Dr. Croll and Mr. Geikie, should have been formed under a subtropical temperature, "have yielded abundance of marine shells which show a much colder set of conditions than those actually inhabiting our present seas." The climatic conditions of the Glacial period may, he thinks, when more fully understood, be explained on geographical grounds without having recourse to the astronomical theory as elaborated by Dr. Croll.

Memoirs of the Geological Survey of India. Vol. xiii., Parts 1 and 2. Calcutta: Government Survey Office. London: Trübner and Co.

THIS volume consists of an account of the Wardha Valley Coalfield. The first notice of the existence of coal in the locality dates as far back as 1831. Twenty-three years later, Mr. Hislup, one of the pioneers of Indian geology, examined the valley. The coal-field, "as limited by an arbitrary line to the south, and by its natural geological boundaries to the east, west, and north, covers an area of about 1600 square miles, and is included between latitudes 19° 28′ and 20° 27′ N. and longitudes 78° 50′ and 79° 45′ E. The district, as suspected by Mr. Blanford and subsequently fully proved by Mr. Fedden, bears evident marks of glaciation. "A boulder-bed, containing some beautifully polished and scored boulders, rests upon a floor of compact Vindhyan limestone, which, when freshly exposed, is found to be striated and grooved in long parallel lines, in the manner so familiar to glacialists." This observation was made "near the little village of Irai, on the right bank of the Pém River, not quite a mile above its confluence with the Wardha, and 10 miles W.S.W. of Chánda." In the Wardha Valley numerous beds of coal occur, one of them 60 feet in thickness. Some of the best, however, are very limited in area. Still the amount of coal is vastly in excess not merely of the present, but even of the probable future wants of the country, both for manufactures and domestic and naval purposes.

Part 2 gives the geology of the Rajmehal Hills, a country bounded on the north and partially on the east by the Ganges, on the south by the Dwarka River and the district of Birbhum, and on the west by the hilly country and the plains of Birbhum and Bhagulpur. One of the most singular geological features of the district is the radiating columnar trap figured in Plate IV., which presents the appearance of pillars radiating out in all directions from a common centre. The region, calmly considered, is pronounced "one of many in India where properly organised commercial enterprise may fairly expect to achieve a reasonable amount of success. There is a total area of about 1200 square miles of coal-field, containing on the lowest estimate 210 million tons of available coal, not of the best quality, but easily accessible. The basaltic trap yields agates, chalcedony, common opal, and various kinds of rock crystal in abundance, but no attempts have yet been made to collect them for commercial purposes.

The Physical System of the Universe, an Outline of Physiography. By SYDNEY B. J. SKERTCHLY, F.G.S., H.M. Geological Survey. London: Dalby, Isbister, and Co. 1878.

THIS is one of the most comprehensive works upon Physiography that has yet appeared. The excellent treatises of Mrs. Somerville and Prof. Huxley are "too much of the earth, earthy," and treat our tiny globe with an excessive amount of consideration. Mr. Skertchly, on the other hand, takes a much broader view of the subject, and refuses to look upon our earth as being the central point of the Universe, regarding it as nothing more than an infinitesimal portion of one vast whole. As might be expected, therefore, the larger half of the book deals with facts and principles which affect the whole of creation, the rest being devoted to the description of the earth, which is looked upon more in the light of a unit in the solar system than as a habitation for man. Mr. Skertchly travels far and wide, taking his reader with him from atoms and molecules to suns and systems.

The author is evidently possessed of the broadest of views in scientific matters, and seems ever ready to seize hold of the newest facts and theories, and lay them before his readers in a digestible form. The latest discoveries relative to the otheoscope and radiometer, for instance, are described at length, and the important light thrown by them upon the modern theory that light, heat, and chemical action are different effects of the same kind of wave-motion, is well brought out. In connection with this theory Mr. Skertchly proposes that we should get rid of all such terms as undulations, vibrations, and waves, and use the neutral term *undæ* for waves of all periods, no matter whether they manifest their existence by giving rise to luminous, heating, or chemical effects. The adoption of some such term would clear away many of the paradoxical expressions which are now used by authors and lecturers, such as radiant heat,—which is not heat until it falls on some particular kind of matter,—dark heat, actinic rays, and so on.

The sun, as may be imagined, receives a large amount of consideration. The latest researches into the constitution of comets are well described; and the way in which recent experiments with the radiometer and otheoscope, and Tyndall on "Amyl Nitrite Clouds," bear on this interesting but mysterious subject is lucidly explained.

Mr. Skertchly writes in a plain lucid style, and never disdains to use a commonplace word or illustration when it will serve to make his explanation plainer. He is an admirable expositor, and irresistibly carries his reader along with him by the charm of his style and the interest which he excites. The book may be read by all classes, from the philosopher and student to the intelligent man of culture who desires to gain some insight into the latest facts and theories relating to the physical constitution of the Universe.

Ure's Dictionary of Arts, Manufactures, and Mining. Vol. iv. Supplement, by ROBERT HUNT, F.R.S. London: Longmans and Co. 1878.

THE editor of a technological work like Ure's well-known Dictionary must have a thankless task, and may be compared to an unsuccessful Hercules trying to stay a perennial hydra. In the present instance Mr. Hunt and his able assistants may be congratulated on the large amount of success with which they have been able to lay before their readers a succinct description of the progress made by modern manufacturers up to the end of last year, and even beyond it. The anxiety shown by the editor to give the latest possible information on every subject is evinced by the fact of his having deemed it necessary to publish an Addendum to his Supplement, for the sake of giving us the discoveries of MM. Pictet and Cailletet, and the newest forms of telephone. It must have been with a slight feeling of mortification that Mr. Hunt must have welcomed the discovery of the microphone, just too late to be included in the new volume.

The system of publishing periodical supplements to such works is an excellent one, and has already been adopted with great success in the case of the sister book, "Watts's Dictionary of Chemistry."

The merits of "Ure's Dictionary" are so well known that we need do no more in the way of praise than to say that the present volume is not only on a par with the preceding ones, but is even superior to them, on account of the very numerous bibliographical references scattered through the book.

Principles of Machine Construction, &c. By the late EDWARD TOMKINS. Edited by HENRY EVERS, LL.D. Vol. i., text. Vol. ii., plates. London: W. Collins, Sons, and Co. 1878.

THIS work, which forms part of Collins's Advanced Science Series, was left partly unfinished by the author, who died before its completion. His original design has, however, been fully carried out by Dr. Evers, who found he could not improve upon it. The first four chapters are devoted to a course of instruction in geometrical and mechanical drawing as applied to machinery, and are fully illustrated by nearly a hundred cuts and six quarto lithographic plates. The different kinds of motion and its transmission, the various forces, the materials of construction and their treatment, occupy the student's attention during the next two chapters, the remainder being devoted to machines in general and their parts; this portion of the book being illustrated by over two hundred and fifty woodcuts and more than forty plates. The plates in the accompanying Atlas, drawn by the late Mr. Tomkins, are well and clearly executed, and are sufficiently large and detailed to be used as drawing copies for advanced students in Science and Art Classes, to whose attention we cordially recommend these volumes.

Free Evening Lectures, delivered in connection with the Special Loan Collection of Scientific Apparatus, 1876. Published for the Lords of the Committee of Council on Education, by Chapman and Hall, 193, Piccadilly.

THE lectures here published were of necessity somewhat popular in their character. As now presented to the reader they labour under the disadvantage of being unaccompanied by the experiments and objects by which they were illustrated when originally delivered. Still they may be read by the general public with profit, and we trust not without pleasure. We must particularly call attention to the note appended to Mr. G. Carey Foster's lecture on "Electricity as a Motive Power," in which the learned author clearly and conclusively demolishes the vague hopes entertained by many that when our coal-fields are exhausted electricity will, in some way or other yet to be discovered, come to our rescue.

Mr. W. J. Harrison's lecture on "Local Geology, with special reference to that of Leicestershire," is interesting from the lucid manner in which the author describes the gradual changes from *Castanea atavia* to *C. vesca*, as traced by Baron von Ettingshausen, in proceeding from more ancient to more modern formations. Mr. Harrison adds—"If we grant that even one species can change in this way, we shall do all that Mr. Darwin desires."

We regret that the work has not appeared earlier, and that it is unaccompanied by an index.

Proceedings of the Literary and Philosophical Society of Liverpool, during the Sixty-sixth Session, 1876-77. London: Longmans and Co. Liverpool: D. Marples and Co., Limited.

THIS volume contains an account of the papers read and the discussions held at the meetings of the Society. From these we are happy to learn that good work is being done. The Rev. H. H. Higgins, a zealous and able naturalist, has taken a cruise in the West Indian Seas on board the *Argo*, and has brought back and described a large and interesting assortment of sponges, Mollusca, fishes, and cryptogamic plants, which are now lodged in the Liverpool Museum. Several Associates of the Society—among whom we may mention Capt. Perry, Capt. Cawne Warren, Capt. Slack, and Mr. E. Dukinfield Jones—have also contributed observations and collections in various departments of Natural History. A letter from Mr. J. Adams, of Pitcairn Island, read by Mr. J. L. Palmer, R.N., gives an account of the appearance of a sea-serpent on October 15th, 1870. It is represented as from 30 to 40 feet in length, and about a foot or 18 inches in diameter. No improbable or sensational features are ascribed to the animal.

Among the papers read before the Society, and here inserted in full, very few indeed can call for our notice. The Rev. T. P. Kirkman laments to see "crowds of good heads busy with mere observations obtained by rifling sea and land and sky, by dissection of the dead, and vivisection of the tortured living," and wishes that, like himself, they would devote themselves to the discussion of questions which Milton happily represents as engaging the attention of a Literary and Philosophical Society in Pandemonium.

Mr. E. Davies, F.C.S., contributes an interesting paper entitled

"Popular Errors about Poisons." It may, however, be questioned whether he does not go too far in denying the existence of slow poisons whose effects are not perceived until a considerable time after their administration. That death does not necessarily follow immediately after the introduction of a lethal substance into the system is often shown in cases of hydrophobia. Whilst, therefore, we fully join Mr. Davies in repudiating the notion that a poisoner can so regulate the dose given as to ensure the death of his victim at some specific and remote time, we do not think it impossible that a man after having taken a poison may remain perhaps for months in apparent health. Of this we have heard of three painful cases, on what seems to be unimpeachable authority.

Mr. A. J. Mott points out some of the shortcomings of Haeckel's "History of Creation," and contends—erroneously in our opinion—that the disciples of Haeckel are the only logically-consistent Evolutionists. The paper shows extensive reading and acute thought, but to our mind it conveys the impression that Mr. Mott is not a working biologist.

Conferences held in Connection with the Special Loan Collection of Scientific Apparatus, 1876. Chemistry, Biology, Physical Geography, Geology, Mineralogy, and Meteorology. Published for the Lords of the Committee of Council on Education. London: Chapman and Hall, 193, Piccadilly.

This work has been tardy in making its appearance. So much has happened to call public attention in other directions that the Exhibition of Scientific Apparatus of 1876 is now almost forgotten. This book consists of a series of papers or addresses read or delivered by certain eminent men, and followed by some slight discussion, the whole being supposed to be connected with or inspired by the articles then and there exhibited. Though sadly in need of a good index, it contains, as might be expected, no small store of interesting facts and of weighty opinions. Thus Prof. Frankland points out, as an important deficiency in our national appliances for the furtherance of scientific culture, the absence of a museum of chemical products. Every chemist must admit that a collection of this nature, kept well up to the level of the day, would be "of the highest interest both to the student and the investigator." The expense of forming and enlarging such a collection would be smaller than might at first sight be imagined, since there are few discoverers of new compounds who would not think it a privilege rather than a tax to deposit a specimen of their products in a great national collection. Why, then, is Prof. Frankland's suggestion still not acted upon?

That other capitals are also deficient in collections of this kind is a poor apology for London.

Still more important is the paper, read by Prof. Frankland on behalf of Prof. Fremy, on the Endowment of Scientific Research. Our own opinions on this subject have been repeatedly declared; but it is with great satisfaction that we find them essentially shared by a man of science of the standing, authority, and experience of M. Fremy, who has for years been earnestly striving to resuscitate original research in France, and who has even instituted a laboratory in which students who have completed their chemical education are received and allowed to work gratis. We are very happy to find Prof. Fremy's paper thus laid before the British public under the authority of the Educational Committee of the Privy Council.

An interesting fact, "not generally known," occurs in Dr. Gilbert's able paper on "Some Points in the Nutrition of Animals." We learn that on comparing the ox, the sheep, the pig, and man, the proportion of stomach by weight is approximately in oxen 3·2 per cent, in sheep 2·44, in pigs 0·88, and in man only 0·38. The author draws hence the very natural inference that the pig requires a more concentrated and digestible food than sheep and oxen, whilst man, on the other hand, requires a still more concentrated food than the pig, the practical conclusion—unwelcome to vegetarians—being that "man was not made to consume potatoes and cabbages by the bushel."

Some remarks made by Prof. Duncan must be very interesting to all inquirers into the distribution of organic life upon the surface of the globe. He declares that "there is a remarkable isolation, so to speak, of the flora of South-Western Australia. It is isolated from that of South-Eastern Australia, but it is most remarkable in its African affinities, necessitating a belief in the former existence of some land connection between Africa and Western Australia. Again, we find the flora of New Zealand connected with South America and with Eastern Australia." These conclusions, and indeed the facts upon which they repose, seem scarcely to harmonise with the views of Mr. A. R. Wallace and other authorities, based upon a consideration of the distribution of animal life.

There is much other matter in this useful and instructive volume which we should be glad to particularise did space permit. We merely mention the interesting paper by Dr. Cornelius B. Fox, on the Use of Aspirators in Atmospheric Ozonometry; Prof. Donder's memoir on the Velocity of Thought, which when brought to the test is found to be incalculably smaller than was supposed by poets and moralists; and Dr. Royston Pigott's notice of the Microscopic Researches of Dr. Drysdale and the Rev. E. Dallinger. The author remarks that "all conclusions founded upon the non-appearance of animalcules in a given fluid, as seen by microscopes of very great cost and power, fall to the ground unless these peculiar methods are adopted which have now been invented." The bearing of this discovery upon the question of spontaneous generation will at once be recognised.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 2. Washington: Government Printing-Office.

THIS issue includes an Essay which in point of importance, if not of extent, is well worthy to figure as an independent work. Under the tirle "The Geographical Distribution of the Mammalia considered in relation to the Principal Ontological Regions of the Earth, and the Laws that Govern the Distribution of Animal Life," Mr. J. A. Allen criticises the views of Dr. Sclater and Mr. A. R. Wallace,* and expounds in detail a system of his own, of which he published a preliminary notice in 1871, and to which Mr. Wallace refers in his great work. His fundamental principle is that "life is distributed in circumpolar zones under the controlling influence of climate, and mainly of temperature." Quoting from Mr. Wallace the declaration that "the continents, resembling a huge creeping plant with roots at the North Pole, and the matted stems and branches of which cover a large part of the northern hemisphere and send three great offshoots toward the South Pole," he likens the distribution of animal life to that of the land. Near the North Pole the land is comparatively little interrupted, the Eastern and Western Worlds being sundered merely by Behring's Straits. In harmony with this geographical fact we have an Arctic or North Circumpolar realm of animal life whose essential forms are common to the two continents. The further we proceed from the Pole the more we find differentiation increase. Thus Mr. Allen obtains eight primary divisions or "realms"—the Arctic, the North Temperate, the American Tropical, the Indo-African, the South American Temperate, the Australian, the Lemurian, and the Antarctic. Most of these realms are subdivided into "regions," and again into "provinces." The resulting arrangement differs, however, from that of Mr. Wallace less perhaps than might have been at first sight expected, and the same boundary lines are observed in both. Thus, setting the Circumpolar realm aside, Mr. Allen's North Temperate —with its two secondary divisions—coincides with Mr. Wallace's Nearctic and Palæarctic regions. The American Tropical of the one author and the Neotropical of the other differ merely in the circumstance that Mr. Allen has raised its extreme southern portion to the rank of a distinct primary division. The Ethiopian and the Oriental regions of Mr. Wallace, thrown together, form the Indo-African realm of Mr. Allen; or rather, we may say, that he takes the two primary divisions of Mr. Wallace, and reduces them to a secondary rank. Madagascar and the Mascarenes, instead of viewing as a zoological province of Africa, he elevates to the rank of a primary realm—a view with which we feel disposed to agree. Concerning the Australian region the two authorities agree almost entirely as far as boundaries are concerned, though Mr. Allen decidedly refers to his Indo-African realm the Philippine group, which Mr. Wallace leaves doubtful, and also Celebes. As regards the subdivisions of the Australian realm, Mr. Allen includes in his Papuan province not merely New Guinea, with the smaller islands to the west and the south-east, but Australia proper north of 20° S. lat., which he considers here as approximately representing the isotherm of 70° F.

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* See Geographical Distribution of Animals, by A. R. WALLACE.

Into the evidence which the author adduces in support of his classification, and the objections which may be urged against it, space does not at present allow us to enter. He appears to have abandoned his formerly proposed separation of Temperate South Africa as a primary realm because it lies "wholly within the warm-temperate belt, and widens rapidly northward to abut very broadly against the torrid zone." Yet this latter feature belongs no less decidedly to Temperate South America, for which the author still claims the rank of a primary realm. A corollary which can scarcely have escaped the reader is formally stated towards the close of this most interesting treatise:—"The northern circumpolar lands may be looked upon as the base or centre from which have spread all the more recently developed forms of mammalian life, as it is still the bond which unites the whole. Of the few cosmopolitan types that in a manner bind together and connect the whole mammalian fauna of the globe (the Lemurian and Australian Realms in part excepted) nearly all have either their true home or belong to groups that are mainly developed in the northern lands. But if this be true of the mammalia must it not hold good of all the great animal groups, and indeed of plants also? If not, the value of the mammalia as a guide to the determination of zoological regions becomes exceedingly doubtful. But if so, we are led to the conclusion that the dawn of life must have been not in the equatorial, but the arctic regions, and that not merely the first animal forms altogether, but the first of each, at least of the grand divisions of the animal kingdom, must have been there developed.

It is obviously impossible in a brief notice like the present to do justice to a work which will doubtless receive from biologists wide-spread and serious attention. If Mr. Allen's time and duties permit it, he could scarcely confer a more valuable boon upon science than the working out of his hypothesis for the animal world at large.

Records of the Geological Survey of India.

Vol. x., Part 4, contains an account of the Geology of the Mahanadi Basin and its vicinity, by Mr. V. Ball, with a notice of the diamonds, gold, and lead-ores of the Sambalpur district, by the same author. The diamonds appear to be decidedly a thing of the past. About 1856 the right of seeking for diamonds in this district was leased by the Government at the low rate of 200 rupees per annum, but the lessee abandoned the undertaking as unremunerative. The author, however, thinks that the southern channel of the Mahanadi might prove more productive than the northern, which has been the scene of all recorded and traditional explorations. The matrix of the diamonds is probably situate in the sandstones and shales of the Barapahar Hills. Beryl, topaz, carbuncle, amethyst, cornelian, and clear quartz were formerly collected in the Mahanadi, probably derived from the metamorphic rocks.

Gold-washing is chiefly confined to the small jungle streams, but the quantity collected does not appear considerable.

Galena, containing about 12 ozs. of silver to the ton of lead, has been found near Sambalpur, but the investigations made were not on a sufficient scale to decide whether it occurs in remunerative quantities.

The principal papers in vol. xi., part 1, are Mr. Lydekker's Notices of the Fossil Mammals of the Siwalik, and Mr. Blandford's Reply to Dr. Feistmantel on the Palæontological Relations of the Gondwana System.

Report of the United States Geological Survey of the Territories. By F. V. Hayden. Vol. vii. Washington: Government Printing Office.

This volume contains a description of the tertiary flora of the western territories. The descriptions are full, and the illustrations numerous and well executed.

Reports of the United States Geological Survey of the Territories. Vol. xi. Monograph of North-American Rodentia. By Elliott Coues and J. A. Allen. Washington: Government Printing Office.

We have again good cause to congratulate the Staff of the United States Geological Survey on the work they are accomplishing. We have here a most elaborate account of the rodents of North America, arranged in their eleven families. Under each species we find a full account of its specific characters and of its geographical distribution. Tables of measurements, generally made upon a great number of specimens, are appended; the synonymy is given in full; and the characteristics of varieties, if such exist, are pointed out. In addition, the generic and sub-generic peculiarities are explained in detail. The only deficiency which we find is in the habits of the various species, concerning which the information supplied is meagre. The carnivorous nature of some of the Spermophile squirrels is, however, clearly shown—a fact of great importance, and one which proves the necessity of great caution in arguing from the structure of an animal to its habits, and especially its diet.

There is also a synoptical list of the extinct Rodentia of North America, and a most elaborate and valuable bibliography of North-American Mammalia.

Each section of the work is accompanied with engravings showing the skulls of the principal species described. The index is scarcely as full as might reasonably be expected from a work of this character.

Heaven and Hell, or the Divine Justice Vindicated in the Plurality of Existences. Containing a Comparative Examination of the Various Doctrines concerning the Passage from the Earthly Life to Spirit Life; Future Rewards and Punishments; Angels, Devils, &c.: followed by numerous examples of the State of the Soul, during and after Death; being the practical confirmation of the Spirits' Book. By ALLAN KARDEC. Translated from the Sixtieth Thousand by ANNA BLACKWELL. 1878. London: Trübner and Co., Ludgate Hill.

THE volume before us is one of a series explaining and defending the doctrines held by Spiritualists. The first principles of Spiritualism are dealt with in previous works; the one under consideration treating, as its title denotes, of the state of the Spirit after its release from corporeal bondage. Basing his arguments upon the plurality of existences or successive terrestrial incarnations, the author proceeds to consider Heaven and Hell from Christian, Pagan, and Spiritualist standpoints, and endeavours to prove the triumph of Spiritualistic doctrine over those preceding its revelation. The subject is most exhaustively considered, embracing the actual situation of Heaven and Hell, the duration of happiness and misery, the existence of Angels and Demons, Purgatory, &c. Throughout the work is remarkable for its sound argument and practical common sense, and those who are most disposed to disagree with the principles enunciated cannot fail to admire the research and force of reasoning that is displayed. The second portion of the work is devoted to examples of statements made in the earlier chapters, being the revelations of released Spirits, and is chiefly of interest as relating to the future of the suicide and to terrestrial expiations. Suicide, under any circumstances, is denounced as crime, and the fate of the unhappy beings who yield to the temptation is vividly depicted. The translator's commentaries form a valuable addition to the work, which will be appreciated as a scholarly adjunct to Spiritualistic literature.

SCIENTIFIC NOTES.

By far the most prominent event of the quarter has been the discovery of the Microphone. The scientific world had scarcely recovered from the surprise excited by the wonders of the Telephone and Phonograph, when Prof. Hughes, the well-known electrician, announced that the electrical resistance of certain bodies was influenced by sonorous vibrations propagated in their vicinity, just as selenium is influenced by light, and that in so supreme a degree as to magnify sounds of the most delicate nature to such a pitch that the tramp of a fly on a wooden box could be distinctly heard. The way in which Prof. Hughes discovered this important fact was the following:—He was trying some experiments on the changes which take place in the electrical resistance of a strained wire, using for the purpose a closed circuit, including a small battery and a Bell Telephone. He noticed that, although no change was apparent when the stretched wire was spoken to, when it broke a rush of sound was heard in the receiving Telephone, the noise being repeated when the wires were re-united. By simply crossing the broken wires, and placing a small weight on the top of them, it was found that sounds uttered close to them were repeated in the Telephone. Other conducting materials were tried with similar effect, some of them—such as gas carbon and pine charcoal—being more sensitive than others. The problem solved by Prof. Hughes's discovery is to introduce into an electrical circuit an electrical resistance which shall vary in exact accord with sonorous vibrations, which are thus transformed into electrical vibrations, to appear once more as sound in the ear-piece of the Telephone; in other words, the molecules of conducting bodies in molar contact and under pressure are so regulated in their motions by the waves of sound in their neighbourhood that they decrease and increase the resistance of a circuit to a remarkable degree. A great variety of Microphones were constructed by Prof. Hughes, of all kinds of materials, each having a special range of variation of resistance. Thus a simple stick of charcoal resting on another is so sensitive that a fly's tramp may be distinctly heard; but it would be too delicate for the human voice, the vibrations caused by this being too powerful. In transmitting articulate sounds the area and the number of the points of contact must be increased so as to reduce the extreme sensibility of the instrument. For magnifying the human voice Prof. Hughes uses a Microphone consisting of a glass tube containing several cylinders of pine or willow charcoal in close contact, the two end pieces being in circuit. Mr. Blyth, F.R.S.E., has succeeded in making a most efficient articulating Microphone out of a jam-pot, or a wooden box containing gas-cinders: electrical continuity is insured by the ends of the circuit being connected with two tin plates thrust between the cinders and the side of the vessel. In another experiment he found that by wetting the cinders he could do away with the battery, while, on the other hand, by using a stronger battery he found that the cinder boxes would act both as receivers and transmitters, thus doing away with the necessity for using a magnetic Telephone. The sounds in the latter case were very faint, and not easily distinguished; but the fact remains that they were transmitted, nothing being in circuit but a couple of cinder boxes and two Grove's cells. Hughes's Microphone has already been applied most successfully as a test for the presence of stone in the bladder and foreign solid bodies in the animal tissues. Experiments of a very promising nature have also been made with the Microphone as a substitute for the Stethoscope in lung and heart disease. Mr. W. J. Lancaster, F.C.S., has rendered the Microphone still less complicated, by doing away with the subsidiary battery, and using the negative plate of a simple carbon and zinc pair as the resonator. Mr. Lancaster's Pile Microphone, as he calls it, consists of a thin wooden box, 6 ins. × 4 ins. × 1 in., upon the top of which is screwed a plate of zinc, 4 ins. × 2 ins., and about one-tenth of an inch thick, somewhat smaller than the top of the box. On this is placed a layer of blotting-paper soaked in dilute sulphuric acid, and on this again a gas carbon-plate, ½ an inch thick; a second, but smaller and thinner, plate of carbon is suspended over the first, at about 2 inches distance, by being screwed to an upright which passes through the plates of the pile into the box below. Against the suspended plate rests a thin upright rod or plate of carbon, the lower end of which is pointed, and rests in a depression scooped out of the lower plate and filled with mercury. Mr. Edison, the inventor of the Phonograph, lays claim to the priority of discovery, on account of his having discovered that the compression of a piece of black-lead interposed in a circuit proportionately altered its electrical resistance, but any discussion as to the merits of the rival discoverers would at present be premature and out of place.

At the March meeting of the Physical Society a new battery of extraordinary capabilities was exhibited and described by Mr. W. H. Preece. It was specially devised by Dr. Byrne to be used by medical men in certain cauterising operations. It consists of a simple cell of platinum and zinc, excited by a mixture of potassium bichromate, sulphuric acid, and water. The negative plate is compound, being backed with lead to which is soldered a sheet of copper, which is again covered with lead. The lead back is covered with a non-conducting varnish, the platinum surface being of course left free. While in use a stream of air is forced through the liquid by a small air-pump, stirring it up actively, the heating power being thereby largely increased. The action of the air is proved to be entirely mechanical, and seems to work by exposing the zinc plate to successive layers of fresh acid. With an 18-inch inductorium sparks of over 17 inches could be obtained from a large 10-cell battery, but immediately the current of air was stopped the spark fell to 8 inches. With a battery of four cells, measuring 4 inches by 2 inches, a length of 6 inches of platinum wire, No. 18 (0·05 in.), was heated to bright redness. With the larger 10-celled battery no less than 30 inches of No. 14 were heated.

At another meeting of the same Society Prof. S. P. Thompson exhibited and described a cheap and efficient form of optical bench. Two straight oak bars, about 2 metres in length, and clamped together as in a lathe-bed, and a number of slides carrying various appliances slide easily without shake, and can be fixed in any position by wedges. The several frames carrying the diffraction grating or edges, the eye-piece (with an engraved glass micrometer), &c., are so made, in wood, as to be capable of adjustment in any plane, and the instrument can also be employed for making photometric and other similar measurements. The mean of two determinations for the wave-length of certain red light gave 0·000629 as compared with Fresnel's figure, 0·000640, while the total cost did not exceed £3.

The following method of precipitating the gold contained in old toning-baths is proposed by F. Haugh. The baths when no longer fit for use are filtered into a white glass flask, rendered alkaline with a little bicarbonate of soda, and a concentrated alcoholic solution of magenta is added drop by drop until the liquid has taken the deep red hue of syrup of raspberries. The flask is then exposed for six to eight hours to the light of a bright window. At the end of this time the gold is found to be deposited as a violet powder, whilst the supernatant liquid has become colourless. It is carefully decanted, so as to preserve merely the deposit. When a sufficient quantity of protoxide of gold has thus been collected, it is carefully washed upon a filter, dried, and the filter is burnt. The dry residue and the ash of the filter are then dissolved at a gentle heat in an excess of *aqua regia*, and the solution—diluted with distilled water—is separated from the insoluble substance by filtration.

For the restoration of writing effaced by time M. E. von Bibra proposes to moisten the writing with a moderately concentrated solution of tannin, the excess of which is then removed by the application of the washing-bottle, and the paper dried at 65°.

The same gentleman has also given, in the *Bulletin de la Societe Chimique de Paris*, a process for seasoning new casks. He proposes to eliminate all soluble matter from the interior of the staves by the use of crystals of soda, of which 1 kilo. is used per hectolitre of the contents. The cask is first filled two-thirds full of clean water, the proper quantity of solution of soda is added, and after the liquid is mixed the cask is filled to the bung. After standing for ten or twelve days the alkaline liquid is run out, and the cask repeatedly rinsed with clean water.

Dr. J. H. Gladstone has examined some candles which are stated to have been recovered from the wreck of a Dutch vessel sunk in Vigo Bay during the war in the year 1702. As the vessel was supposed to be a treasure ship many attempts have been made to recover its contents, and this was successfully accomplished in 1875, when these candles among other things were obtained. They have therefore been submerged for 173 years. The wick has rotted away, leaving scarcely any trace of its existence, while the fatty portion has become a friable heavy substance, of a dull white colour. The candles bore evidence of having been made by dipping, for the concentric layers were easily separated from one another, and this facilitated the examination of the outer and inner portions of the same piece. Both the outer and inner portions still contain some of the fat apparently unchanged; they are unctuous to the touch, and have a fatty odour, and when heated to a little below 110° C. they began to change colour; at 140° they softened, and at 200° small portions were melted out. The most interesting point is, that whereas the fats have been in contact with a practically unlimited amount of sea-water for 173 years, and a chemical change between them has been possible, the double decomposition has proceeded so extremely slowly that the reaction is only about half completed at the present time.

Mr. T. A. Readwin records a curious instance of spontaneous metal-growth. About twelve years ago he put about an ounce of rolled metallic cadmium (coiled six times) into a clear flint-glass phial, corked it tightly, cut the cork level with the mouth of the phial, and sealed it very carefully. At the time the metal did not quite reach up to the neck of the phial. Since that time it has elongated more than a quarter of an inch, and, during the present year, the sealing wax has been broken all round and the cork forced outward more than one-eighth of an inch. The cork is even now fairly tight, but the metal follows after it to touching forcibly, and probably the cork will be presently completely forced out. The cadmium is fast oxidising and endeavouring to uncoil itself, and the cut edges at the bottom of the phial are becoming foliated.

We have received a number of biscuits and other preparations containing preserved solid and liquid food, both animal and vegetable, which are the practical results of a new process lately patented by Dr. Campbell Morfit. They include substances of the most diverse nature, such as milk, cream, cheese, beef, garden rhubarb, cabbage, tomato, pork sausage, and a variety of other alimentary products, all of which are perfectly savoury and toothsome, in spite of their being more than a year old It is, however, more with Dr. Morfit's process than with its present results that we have now to deal, for we must look upon his discovery as being yet in its infancy. Dr. Morfit's experiments, which he has prosecuted uninterruptedly for the last two years, seem to prove that ordinary gelatin, when it is once thoroughly diffused through a vegetable or animal substance, and dried in and with it, will protect it from decomposition or other alteration for a prolonged period, in spite of atmospheric or climatic changes. This is clearly proved by the samples submitted to us, which—although they have been exposed to the constant changes of temperature and moisture consequent on their having been kept for more than a year in the store-room of an ordinary dwelling-house—are still perfectly good and sweet, their natural characteristic flavours being well preserved. Some lime-fruit juice biscuits, for instance, which are more than a year old, have preserved, in a very perfect manner, the peculiar flavour by which the juice of the lime can always be distinguished from that of the lemon. The primary principle of Dr. Morfit's process is the getting rid of nearly the whole of the natural water contained in the substance to be preserved, by submitting it to a certain degree of heat, the place of the water being supplied by gelatin. The compound is then dried, and in this state may be kept for any length of time, or else it may be made up into biscuits by incorporating it with biscuit-powder. Let us take Dr. Morfit's method of preserving beef as an example. The beef must be as free from fat and bone as possible, and should be first stewed in its own liquor, or with the least possible quantity of water, and seasoned or not according to taste. The whole is then reduced, by any available mechanical means, to a state of smooth and fine pulp, and triturated with a solution of gelatin in water. One pound of gelatin is enough for 15 pounds of meat, fowl, or fish, the gelatin being dissolved either in a sufficiency of water or in the natural juice of the substance itself. In the case of fruit—such as gooseberries, currants, or plums—they are stoned or skinned when necessary, and cooked or not, as the case may be. They are then made into a pulp and mixed with gelatin dissolved in water or their own juice, heated so as to insure a thorough mixture of the ingredients, and then poured into coolers. In certain cases the gelatin may be replaced by mucilage of Irish moss, but the result, although cheaper, is not so good. Dr. Morfit's method of condensing milk without the use of sugar is of great interest, seeing that the Swiss and other descriptions of condensed milk, which are now so largely sold, cannot be taken by delicate infants or by persons of weak digestion, owing to the large amount of sugar in them. One pound of gelatin is dissolved in one gallon of fresh milk at a temperature of from 130° to 140° F., the whole being allowed to set into a jelly, which is dried. The dried jelly is then dissolved in another gallon of fresh milk, and allowed to set and dry as before, the operation being repeated with fresh milk until the original pound of gelatin has taken up eight gallons of milk or more. Consommé of meat may in like manner be condensed until one pound solid shall represent thirty times its weight of fresh beef. As may be readily guessed, the process may be carried on without any of the expensive plant and troublesome manipulation involved in the usual modes of condensing milk and making Liebig's extract, besides which, in the latter case, the whole of the nitrogenous parts of the meat are preserved intact. From a hygienic point of view, the lime-fruit juice biscuits ought to be admirably suited for use in the Navy. Without entering into the question as to whether it is the citric acid, or the phosphatic salts, or the potash, contained in the lime-juice that is the real anti-scorbutic agent, it is sufficient to say that the 40 per cent. of Montserrat lime-fruit juice preserved by Dr. Morfit's process, and incorporated with the biscuits, has preserved all its properties without any change for more than a year, and, *à priori*, there is no reason to suppose that it would not keep good for ten or twenty times that period. It may be mentioned, in conclusion, that the different jellies may be dried into hard tablets or flakes at a uniform temperature of from 38° to 40° C., and sent into the market in this convenient form, as well as under the more bulky guise of biscuits. A few cases of lime-fruit juice tablets, prepared according to Dr. Morfit's method, would probably have saved the lives of several brave men during the late expedition to the Polar regions. Speaking from a purely scientific point of view, and judging by the results we have already described, the principle of Dr. Morfit's invention seems to be theoretically a sound one. These results we must regard at present as tentative, and it only remains to the inventor of the process to confer a large benefit on the community by extending its application, thereby notably increasing our not too abundant stock of hygienic and alimentary products.

The Ross microscope, as remodelled by Mr. F. H. Wenham, and described in the "Quarterly Journal of Science" for 1873 (p. 422), has again been greatly improved by the same ingenious gentleman. The general form of the stand resembles the former one, but the weight has been diminished without in any way interfering with its stability. The stage is also somewhat lower, rendering manipulation more convenient. The fine adjustment no longer acts upon an adapter in the nozzle, but bears directly upon a slide carrying the whole body of the instrument. This contrivance gives great accuracy and steadiness of movement, and prevents the alteration of magnifying power inseparable from the older contrivance, and which gives great trouble when micrometers are in use. The stage has been simplified and rendered extremely thin, without sacrificing any useful movement. Below the stage the American swinging bar has been adopted, allowing the mirror, with the whole of the very simple yet effective sub-stage apparatus, to be moved out of centre, even to the extent of being placed *above* the stage, while its accurate placing in the axes of the microscope when required is secured by means of a clamping-screw. The scope offered for the employment of great variety of oblique illumination by simple contrivances, such as spare objectives and eye-pieces used as condensers, will suggest itself, while the microscope will still carry every other kind of illuminator. Notwithstanding these advantages, the simplification in construction has enabled the stand to be made at 25 per cent less cost than its predecessor.

The adjustment collar, a source of much trouble to the less expert observers with high powers of large angular aperture, has been dispensed with in an objective constructed by Herr Zeiss, of Jena, at the suggestion of Mr. J. W. Stephenson, F.R.A.S., from the formula of Prof. Abbe. The objective is on the immersion principle, and depends for its properties upon a fluid being employed having the same refraction and dispersion as crown glass. After many trials it was found that oil of cedar-wood gave perfect definition with oblique light, but for central illumination was greatly improved by the addition of one-fourth or one-fifth of oil of fennel seed (*Ol. Fœniculi*). The objective has a balsam angle of 113° = 1·25 numerical aperture.* It has a large working distance. The space between the front lens and the object is 0·02, which gives a working distance 0·012 for 0·008 cover-glass, 0·016 for 0·004, and so on. Its power is rather more than one-ninth, and, having component lenses throughout the combination larger than in other objectives of the same power, it transmits more light.

Two numbers of the "Journal of the Royal Microscopical Society" have been issued. Although the amount of printed matter is less than under the former arrangement, and the issue is only bi-monthly instead of monthly, the Fellows of the Society are decidedly gainers by the change, as—the Council now having entire control over the publication of their Transactions—a quantity of controversial matter, which detracted greatly from the status of the former journal, is rigidly excluded. Besides papers read before the Society, the Journal contains a well-compiled abstract of Microscopy from foreign and other sources. It also contains a list of articles on microscopical subjects, published in various British and foreign periodicals.

The Atlas of Colorado, issued by the United States Geological Survey of the Territories under Prof. F. V. Hayden, embodies the results of the geological and geographical work of the Survey during the years from 1873 to 1876 inclusive. This atlas will contain the following maps:—1st. A general drainage map of Colorado on a scale of twelve miles to the inch. 2nd. An economic map of the same region, having as its basis the above-mentioned drainage map. This map will indicate the areas of arable, pasture, timber, coal, mineral, and desert land in as great detail as possible on the scale. 3rd. A general geological map on which the areas covered by the principal formations will be shown. The drainage map will form the basis for this also. 4th. A map showing the scheme of the primary triangulation in the State. Scale twelve miles to the inch. 5th. Six topographical sheets, showing the same area as that covered by the general drainage map, but in much more detail. The scale of these sheets is four miles to an inch. The relief of the country is indicated by contour lines, at vertical intervals of 200 feet. The area covered by each of these sheets is 11,500 square miles. 6th. Six geological sheets, of which the basis are the six topographical sheets just mentioned. On these the detailed geology is expressed by colours.

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* Numerical aperture is product of the sine of the semi-aperture with the refractive index of the medium in which the observation is made (*vide* "Description of Professor Abbe's Apertometer," Trans. Roy. Micr. Soc., Dec. 5, 1877, vol. i., p. 19).

The "Mineralogical Magazine," which contains the proceedings of the Mineralogical Society, in addition to other interesting items about minerals, has now attained its second year, and, judging by its contents, seems likely to have many years of prosperity before it. Chemists, geologists, and mineralogists alike will find valuable matter in its pages. Judging by the balance-sheet, the Mineralogical Society appears to be growing in numbers and worldly possessions.

Captain Burton's expedition to the Land of Midian seems to have done a large amount of good work in a short time. After four months' absence, they brought back an immense number of entomological and other specimens with them, 25 tons of which were illustrative of the geology and mineralogy of the districts through which they passed. The country seems a rich one. Several gold deposits of an easily workable nature have been discovered in the south of Midian, while in the north there are silver and copper deposits of great value. Three turquoise mines were found, of which one is being roughly worked, besides sulphur beds, rock salt, two salt lakes, gypsum mines, and alabaster quarries. Part of the ores will be retained in Cairo for analysis, and the rest sent to London and Paris. Colonel Gordon's expedition through the Khedive's newly acquired territory in Southern Egypt has also come across several auriferous and argentiferous deposits, specimens of which have been sent to M. Daubrée, the director of the Paris School of Mines, for analysis.

The Hygienic Society of Paris has arranged with Captain M. Giffard for the performance of a number of scientific experiments on the effects of diminished barometric pressure on respiration and other vital processes by the aid of his giant captive balloon. We are glad to hear that this magnificent machine is to be put to a scientific use.

The French Chamber of Deputies has voted a sum of 690,000 francs for the construction of a new astronomical observatory at Meudon, near Paris, on the site of the old chateau which was destroyed by the Germans in 1871. Of this munificent sum, 390,000 francs is to be spent on a refractor, 250,000 francs on the building, and the rest in extra salaries and incidental expenses. The whole is expected to be completed within two years. The separation of the staff of the astronomical and meteorological departments has been decided upon, and, taking advantage of the good example set them, the Swedish Diet has granted the sum necessary for building a separate meteorological observatory at Upsala, where so much good work has been done.

We regret to have to announce the death of the secretary and director of the Smithsonian Institution, Washington, D.C., Joseph Henry, LL.D., which occurred on Monday, May 13th. Professor Henry was born in Albany, in the State of New York, December 17th, 1799. He became Professor of Mathematics in the Albany Academy in 1826; Professor of Natural Philosophy in the College of New Jersey, at Princeton, in 1832; and was elected the first Secretary and Director of the Smithsonian Institution in 1846. He received the honorary degree of Doctor of Laws, from Union College, in 1829; and from Harvard University in 1851. He was President of the American Association for the Advancement of Science in 1849; was chosen President of the United States National Academy of Sciences in 1868; President of the Philosophical Society of Washington in 1871; and Chairman of the Light-House Board of the United States in the same year; the last three positions he continued to fill until his death. Professor Henry made contributions to science in electricity, electro-magnetism, meteorology, capillarity, acoustics, and in other branches of physics; he published valuable memoirs in the transactions of various learned societies of which he was a member; and devoted thirty-two years of his life to making the Smithsonian Institution what its founder intended it to be, an efficient instrument for the "increase and diffusion of knowledge among men."

At a Special Meeting of the Board of Regents of the Smithsonian Institution, held on May 17, 1878, Professor Spencer Fullerton Baird, for many years the assistant secretary of the Institution, was duly elected as the Secretary of the Smithsonian Institution, to succeed the late Professor Joseph Henry.

We are indebted to Prof. F. V. Hayden for an account of the field-work of the United States Geological and Geographical Survey of the Territories, under his direction, for the season of 1877. From it we learn that in 1874 the photographic division of the United States Geological Survey was instructed, in connection with its regular work, to visit and report upon ruins of an extensive and interesting character, which were known to exist throughout New Mexico and Arizona, and in pursuance of this object made a hasty tour of the region about the Mesa Virde and the Sierra el Late, in South-western Colorado, the results of which trip, as expressed by Bancroft, in the "Native Racesof the Pacific Coast," "although made known to the world only through a three or four days' exploration by a party of three men, are of the greatest importance." The following year the same region was visited by Mr. W. H. Holmes, one of the geologists of the Survey, and a careful investigation made of all the ruins. Mr. Jackson, who had made the report the previous year, also revisited this locality, but extended his explorations down the San Juan to the mouth of the De Chelly, and thence to the Moqui villages in Northeastern Arizona. Returning, the country between the Sierra Abajo and La Sal and La Plata was transversed, and an immense number of very interesting ruins were first brought to the attention of the outside world by the report which was published the following winter by Messrs. Holmes and Jackson, in the survey, vol. ii., No. 1. The occasion of the Centennial Exhibition at Philadelphia led to the idea of preparing models of these ruins for the clearer illustration of their peculiarities, four of which were completed in season for the opening of the Exhibition. A study of the models give a very excellent idea of the ruined dwellings themselves. The first of these models, executed by Mr. Holmes, with whom the idea originated, represents the cliff house of the Mancos Canon, the exterior dimensions of which are 28 inches in breadth by 46 inches in height, and on a scale of 1·24, or 2 feet to the inch. This is a two-story building, constructed of stone, occupying a narrow ledge in the vertical face of the bluff 700 feet above the valley, and 200 feet from the top. It is 24 feet in length and 14 feet in depth, and divided into four rooms on the ground-floor. The beams supporting the second floor are all destroyed. The doorways, serving also as windows, were quite small, only one small aperture in the outer wall facing the valley. The exposed walls were lightly plastered over with clay, and so closely resembled the general surface of the bluff that it becomes exceedingly difficult to distinguish them at a little distance from their surroundings. The second model of this series was constructed by Mr. Jackson, and represents the large "cave town," in the valley of Rio de Chelly, near its junction with the San Juan. This town is located upon a narrow bench, occurring about 80 feet above the base of a perpendicular bluff some 300 feet in height. It is 545 feet in length, about 40 feet at its greatest depth, and shows about 75 apartments on its ground-plan. The left-hand third of the town, as we face it, is overhung some distance by the bluff, protecting the buildings beneath much more perfectly than the others. This is the portion represented by the model. A three-story tower forms the central feature; upon either side are rows of lesser buildings, built one above another upon the sloping floor of rock. Nearly all these buildings are in a fair state of preservation. This model is 37 by 47 inches, outside measurements, and the scale 1·72, or 6 feet to the inch. A "restoration" of the above forms the third in the series, of the same size and scale, and is intended, as its name applies, to represent as nearly as possible the original condition of the ruin. In this the approaches were made by ladders and steps hewn in the rock, and the roofs of one tier of rooms served as a terrace for those back of them, showing a similarity, at least, in their construction to the works of the Pueblos in New Mexico and Arizona. Scattered about over the buildings are miniature representations of the people at their various occupations, with pottery and other domestic utensils. The "triple-walled tower," at the head of the McElmo, is the subject of the fourth model. It was constructed by Mr. Holmes, and represents, as indicated by its title, a triple-walled tower, situated in the midst of a considerable extent of lesser ruins, probably of dwellings, occupying a low bench bordering the dry wash of the McElmo. The tower is 42 feet in diameter, the wall 2 feet thick, and now standing some 12 feet high. The two outer walls inclose a space of about 6 feet in width, which is divided into 14 equally-sized rooms, communicating with one another by small window-like doorways. The next is a "cliff-house" in the valley of the Rio de Chelly. It is about 20 miles above the cave town already spoken of. This is a two-storey house, about 20 feet square, occupying a ledge some 75 feet above the valley, and overhung by the bluff. The approach from the valley is by a series of steps hewn in the steep face of the rock; and this method was the one most used by the occupants, although there is a way out to the top of the bluff. This model is 42 inches in height by 24 broad, and is built upon a scale of 1·36. Téwa, one of the seven Moqui towns in North-eastern Arizona, is a very interesting and instructive model, representing, as it does, one of the most ancient and best authenticated of the dwellings of a people who are supposed to be the descendants of the cliff-dwellers. Téwa is the first of the seven villages forming the province as we approach them from the east, and occupies the summit of a narrow mesa some 600 feet in height and 1200 yards in length, upon which are also two other somewhat similar villages. The approach is by a circuitous road-way hewn in the perpendicular face of the bluff which surrounds the mesa upon all sides. It is the only approach accessible for animals to the three villages. Other ladder-like stairways are cut in the rock, which are used principally by the water-carriers, for all their springs and reservoirs are at the bottom of the mesa. This village is represented upon a scale of 1 inch to 8 feet, or 1·96. The dimensions of the model are 36 inches in length, 29 inches in width, and 14 inches in height. In the spring of 1877 Mr. Jackson made a tour over much of the northern part of New Mexico, and westward to the Moqui towns in Arizona, and secured materials for a number of very interesting models, illustrating the methods of the Pueblos or town-builders in the construction of their dwellings. Two villages have been selected for immediate construction, as showing the most ancient and best known examples of their peculiar architecture, viz: Taos and Acoma; the one of many-storied, terraced houses, and the other built high up on an impregnable rock. The model of Taos is now completed, the dimensions of which are 42 by 39 inches, and the scale one inch to twenty feet, 1:240. Of this town Mr. Davis says: It is the best sample of the ancient mode of building. Here are two large houses three or four hundred feet in length, and about one hundred and fifty feet wide at the base. They are situated upon opposite sides of a small creek, and in ancient times are said to have been connected with a bridge. They are five and six storeys high, each story receding from the one below it, and thus forming a structure terraced from the top to bottom. Each storey is divided into numerous little compartments, the outer tier of rooms being lighted by small windows in the sides, while those in the interior of the building are dark, and are principally used as store-rooms. * * * The only means of entrance is through a trap-door in the roof, and you ascend from storey to storey by means of ladders on the outside, which are drawn up at night. Their contact with Europeans has modified somewhat their ancient style of buildings, principally in substituting doorways in the walls of their houses for those in the roof. Their modern buildings are rarely over two storeys in height, and are not distinguishable from those of their Mexican neighbours. The village is surrounded by an abode wall, which is first included within the limits of the model, and incloses an area of eleven or twelve acres in the extent. Within this limit are four of their *estufas* or secret council-houses. These are circular underground apartments, with a narrow opening in the roof, surrounded by a palisade, ladders being used to go in and out. These models are first carefully built up in clay, in which material all the detail is readily secured, and are then cast in plaster, a mould being secured by which they are readily multiplied to any extent. They are then put in the hands of the artists and carefully coloured in solid oil paints to accurately resemble their appearance in nature, and in case of restorations or modern buildings, all the little additions are made which will give them the appearance of occupation. The Survey is in possession of the data for the construction of many more models, and they will be brought out as opportunity is given. They have also, in connection with the views, multiplied many of the curious pieces of pottery which have been brought back from that region by the various parties connected with the survey.

Mr. J. J. Maclaren writes to ask us to correct an error which occurred in the review of his work on the "Chemical Difficulties of Evolution," which appeared in the April number of this journal. He says:—"I am *not* the author of the work on Darwin by James Maclaren (published in 1876), to which your reviewer refers in his article, and, in so far as he has censured the author of that work, he has been under a mistake, and has done that gentleman an injustice. If your reviewer will look at the title-pages and the prefaces of the two books, he will at once see the mistake which he has made. As it is, the similarity of name, and the accident that the excellent printing and paper of the book to which your reviewer refers led me to employ the same publisher, have led him into an error of which I am bound to take notice, and which I am confident you will wish to correct at once."